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XUM

THE

JOURNAL OF SCIENCE.

FEBRUARY, 1883.

I. NEW RESEARCHES ON CANINE MADNESS.

By Frank Fernseed.

AMONG the sources of peril to which mankind expose themselves rabies, or, as it is popularly called, canine madness, holds a somewhat exceptional position: one of its worst features is its uncertainty. The cobra is always dangerous, and whenever we see one we may therefore avoid it, or kill it if we have the means, no man saying us nay. If bitten we are not left in uncertainty, but in a couple of hours at the most we know the worst. Further, though venomous serpents do occasionally intrude into human habitations, yet we never find them brought purposely into public conveyances. In England, too, we are practically free from the assaults of the death-snakes; for although the common viper (*Berus letalis*) still occurs in our heaths and woods, it is never met with in the streets of towns and villages, or on public roads. Even in its favourite haunts it glides away at the sound or the vibration of an approaching footstep. Thus the chances of any person being bitten by this reptile are exceedingly small. Only one fatal case is known as having occurred in Britain during the last half-dozen years. But the dog, unlike the cobra or the viper, is always with us, and is able—under conditions far from fully explored—to inflict a bite fully as hopeless as that of any serpent. As hopeless, I say, but far more distressing, since the person bitten, provided only his skin has been broken, does not know for months—possibly for years—whether he may not be suddenly attacked with a disease which has hitherto mocked all medical skill.

Nor is this lamentable consummation at all rare among us. The deaths from hydrophobia in England and Wales reach a yearly mean of 35,—a considerable number to be sacrificed to the mere caprice of some of our neighbours.

I am well aware that certain moralists, and a few other writers who might perhaps be styled *im*moralists, maintain that the original cause of rabies must be sought in confinement, chains, and muzzles. To this assertion I am content to oppose the well-known fact that this disease is by no means uncommon among wolves in France, Spain, and other continental countries, and is exceedingly common among jackals and pariah dogs in the East.

Having thus proof positive that increased liberty is no remedy, the public may turn with the greater interest to the recent experiments which have been undertaken by M. Pasteur, with a view to tracing the origin of, and if possible discovering, the remedy for this scourge. I premise that these experiments of necessity involve a certain amount of what is, in Victoria Street, shrieked at as "Vivisection." But even experimental research has been so far beset with difficulties. Hitherto the saliva has been the only matter in which the presence of the virus has been actually proved; but, as M. Pasteur remarks, "the saliva, whether inoculated by a bite, or by direct injection into the cellular tissue, does not communicate the disease with certainty, and when the malady does appear it is only after an 'incubation' of variable and undetermined length." Hence the results of experimentation have been hitherto of little value. Had M. Pasteur been a Lord Chief Justice or a Mænad he might have moralised on the uncertainty of "Vivisection" and have abandoned the study of rabies in despair. Fortunately he was a man of Science, and as such he acted on the principle that if any method of experimentation gives doubtful results it must be modified. He found that the principal seat of the poison of rabies is the central nervous system. Here it may be obtained in a state of purity and in sufficient quantity. Further, if this matter is brought in contact either with the surface of the brain or is injected into a vein, rabies is induced not possibly and after the lapse of a longer or shorter time, but invariably and immediately. Thanks to these two observations the disease can now be scientifically studied, and the value of any proposed method of treatment can be rigorously tested. The principal results which M. Pasteur has formally obtained, and which he now puts forward as fully established, are that mute and furious rabies—or rather all the forms of the disease—proceed from one and

the same virus, and that it is possible to proceed experimentally from either of these types to the other.

Nothing is more fluctuating than the symptoms of canine madness. Each case has some characters peculiar to itself, and there are grounds for admitting that these characters depend on the points of the nervous system where the virus is localised.

In the saliva of a rabid animal the virus is associated with various microbia, and the inoculation with this saliva may bring on any one of three kinds of death, *viz.*, death by the new microbion which the author has described as the salivary microbion, death owing to an extensive production of purulent matter, or, lastly, death by what is commonly called hydrophobia. Hence it will be perceived that the bite of a rabid animal is an affair even more serious than is commonly imagined, since a triple danger has to be encountered. I may add that a bite even from an animal in a normal state of health is far more to be dreaded than a wound received from any clean tool or weapon.

The rachidian bulb of a person or of any animal who has died of rabies is always virulent, but the virus may be also found in the whole brain or in some of its parts, or in the spinal marrow. The virulent matter taken from the spinal marrow is quite as deadly as that from the rachidian bulb or from any part of the brain, and the virulence persists until the nervous matter is attacked by putrefaction.

Rabies communicated by the injection of the virulent matter into the blood offers characters which often differ decidedly from the furious madness brought on by a bite, or by placing the virus in contact with the brain. Hence it is probable that many cases of so-called mute madness escape observation.

The author is led to the belief that in inoculations by way of the blood the spinal marrow is the part first attacked. Inoculation in a dog with the saliva or the blood of a rabid animal does not always prove fatal, but such a recovery is no certain protection against the effects of a fresh inoculation with the pure matter of rabies, whether by application to the brain or by injection into a vein.

M. Pasteur has met with cases of spontaneous recovery in dogs after the first symptoms of rabies had appeared, but never after the disease has reached its acute stage. He has also observed cases where the first symptoms had subsided, but the disease had nevertheless reappeared after a long interval (two months) without any fresh infection. Under such circumstances the acute symptoms were followed by

death as in ordinary cases. The question may here be raised whether rabies is always fatal in wild animals, or whether, as certain facts suggest, recovery is not uncommon? M. Pasteur's experiments have not, of course, shown whether idiopathic rabies differs at all in its character from the disease as communicated by inoculation. In one experiment, made in 1881, three dogs were inoculated. Two of these quickly became rabid, and perished, whilst the third, after having manifested the first symptoms, recovered. This last dog was twice re-inoculated in 1882, the poison being brought in contact with the brain, but did not become rabid. Here, therefore, we have a case of the disease in a mild form proving a prevention against future infection. This result M. Pasteur considers the first step towards discovering a means of preservation from rabies. He possesses at present four dogs which are proof against the infection, whatever may be the method of inoculation used and the virulence of the matter; whilst other dogs, inoculated with the same virus, invariably perish. M. Pasteur raises the question whether these four animals owe their impunity to spontaneous recovery from a mild attack which may have escaped observation, or whether they are naturally and exceptionally refractory to the infection? This question he is now endeavouring to decide.

M. Paul Bert has also called attention to some experiments on the same subject which he made in the years 1878 and 1879, and which have been to a great extent overlooked. He effected, on one occasion, the total reciprocal transfusion of the blood between a healthy dog and one in the height of rabies. The former, though kept afterwards under observation for a year, showed no signs of madness, whilst the general condition of the mad dog was somewhat improved, and it survived, apparently, forty-eight hours longer than it would otherwise have done.

M. Bert has also examined in which of the constituents of the slaver of a rabid dog the virus is contained. This slaver is a very complex mixture, containing—in addition to the saliva of the parotid, the maxillary, and sublingual glands—the mucus of the mouth and of the respiratory passages. To decide this question he has inoculated series of dogs with the mucus and with the liquid expressed from the different salivary glands of dogs killed in the height of rabies. The salivas—in the strict sense of the term—have never communicated rabies, whilst the mucus from the respiratory organs has done so, and must be considered as the deadly portion of the slaver. This explains the great

irregularity in the action of the bite of mad dogs, since one bite may convey a portion of the mucus into the wound, whilst another may not.

In accordance with M. Pasteur, M. Bert remarks that the saliva of a mad dog, if it does not communicate rabies, may still prove fatal by producing serious local injuries. Out of fifteen dogs inoculated in 1878 seven were attacked with suppurations, which in four cases proved fatal. Hence the secretions and tissues of rabid animals appear to have septic properties over and above the special rabic virus. If the slaver of a mad dog was filtered through plaster the filtrate was found inoffensive, whilst the residual solid matter communicated rabies.

In connection with this disease there is a question which urgently calls for investigation in some country where such researches are possible. It is said that the bite of the skunk, when the animal is in its normal condition, invariably, or at least generally, brings on rabies. Is this actually the case? If so, does this dangerous property extend to the skunk of Java, or is it peculiar to the American forms? Does idiopathic rabies occur in any other of the Mustelidæ? These questions are of the greater importance as dogs, in attacking weasels, polecats, &c., may easily happen to receive a bite. I have even heard it asserted that rabies does not originate among dogs, but is derived from some animal of the weasel group—a very hazardous assertion. We do not even know whether this disease ever originates spontaneously in any of the Felidæ, Hyænidæ, or Ursidæ.

The recent investigations of MM. Pasteur and Bert lead to certain important conclusions:—

1. The bite of a rabid animal is more dangerous than is commonly supposed, since if it fails to communicate rabies it may occasion other serious affections.
2. The bite of any animal not in a rabid state has to be regarded not as a simple wound, but as more or less poisonous, since the saliva is always liable to introduce microbia into the tissues and the fluids of the animal or person bitten. This conclusion fully agrees with the results of common observation.
3. The fact that a dog which has bitten any person does not itself die of rabies, or does not even exhibit the more marked symptoms of that disease, is no proof that it may not have been slightly rabid, and have communicated the virus to the person bitten.

The question will now be asked, Do the recent studies of M. Pasteur hold out any promise of deliverance from the

perils of canine madness? It seems, *per se*, not unlikely that a "vaccine" for rabies may be discovered, *i.e.*, a modified virus, which if introduced into a dog or any other animal may render it proof against the effects of an infected bite. But should such a safeguard be discovered its adoption, in this country at least, would be very doubtful. The outcry raised by the Anti-Vaccinationists, and in general by the "Anti" portion of the public, would be truly deafening. It has been already shown that much of the evil might be practically done away with by the simple expedient of nipping off the points of the teeth of dogs, so as to render the penetration of the skin less likely. But the dog-worshippers, who are the very heart of the Bestiarian movement, object, and their outcries carry more weight than do considerations of public safety.

II. THE BIRTH AND EVOLUTION OF THE SOLAR SYSTEM.

TRANSLATED FROM PAGES 113 TO 132 OF "GEDANKEN" (1857, Philadelphia, published by Thomas).

By O. REICHENBACH.

Introductory Letter to the Editor.

THE doubts raised in Prof. Klinkerfues's criticism of "Two Planets beyond Neptune" ("Journal of Science" for June, 1882) against the nebular theory as applied by Laplace to the solar system, and the objections of Judge Stallo in his "admirably written" "Concepts and Theories of Modern Physics," made me think of putting before your readers the exposition of a theory which neither fully adopts nor rejects, but modifies and brings into a more definite shape, that of Laplace.

When "it is not the purpose" of Judge Stallo "to discuss the merits" "of the meteoric theory in detail, or to express an opinion as to its soundness and sufficiency," but he yet thinks it "to stand in favourable contrast to the nebular theory," I beg him to consider that a nebula is a gathering

of infinitesimal meteors, and that the nebular theory does not exclude the formation and absorption of larger meteors by a nebular system in formation or formed, and that the "meteoric theory" is a vague inordinate assertion, but no theory at all. I had almost given up my intention, when it was revived by some passages in the Anniversary Address (1882) of Dr. William Spottiswoode to the Royal Society. He said—"On September 17th an observation apparently unique in the history of Astronomy was made by Mr. Gill at the Cape, who watched the comet right up to the sun's limb. . . . It could not, however, be detected in the sun, and this circumstance of appearing neither bright nor dark when in front of the sun appears to suggest a very small substantiality or great separation of the cometary matter."

The comet reappeared with a nucleus, or with more than one nucleus; the mere insubstantiality and division could therefore not have been the sole cause of its imperceptibility. —the refraction of light inside the extended and increased atmosphere on the night side of the nucleus diminishing in size would be in itself an auxiliary reason, but the sentence "The water of the ocean is continually decomposed at the limits of the atmosphere, passing into new combinations again changing in the course of its return: this electromagnetic process, going on around the sun on a grand scale, exists around the planet in a small way" ("On some Properties of the Earth," p. 318) may well apply to a comet near the sun. At all events the comet has presented to various observers appearances and phases the interest in which may perhaps procure some attention from your readers for the following development theory of the solar system.

Dr. Spottiswoode also said—"Nor must I omit mention of Dr. C. W. Siemens's bold and original theory of the conservation of the solar energy, which has already given rise *to so much discussion*. It will be sufficient for me here to say that upon the questions therein raised the last word has been by no means said; and that whether the theory be ultimately established, or whether, like a phœnix, it shall hereafter give rise to some other outcome from its own ashes, it will ever be remembered as having set many active minds at work, and will always have a place in the history of Solar Physics." Dr. Spottiswoode attributes to the theory a novelty it cannot claim, as he may see from a letter, "The Sun," in the "Engineer" of February 26th, 1869, where the views expressed with regard to the maintenance of the "solar energy" differ only in this, from those of Dr. Siemens's theory, that they distinguish between the action of the

individual sun with an atmospheric limitation and the interaction of the various bodies of the whole solar system. The exaggerating way of putting the theory Dr. Spottiswoode may find in "On some Properties of the Earth" (1880):—"We cannot consider the heat emitted, whatsoever may be the manner and kind of its development and nature, as merely the product of the sun, and as being an arbitrary quantity independent of the solar system; we might rather compare the sun to a furnace, consuming the smoke and gases its heat sets free, to feed its own fire and luminary, and to assist in the process of reduction—a metaphor which I do not intend to introduce here as a hypothesis."

The "active mind" which enters into the spirit of my "development theory" may learn that there is a difference between a system forming and formed, and that the process of maintenance of what we may call the seasonal energy of either planet or sun is to be distinguished from the more permanent life process of the system.

Translation.

Although in the important distribution of planets general qualities of absolute magnitude, density, depression, velocity of rotation, and absence of moons show themselves as dependent from their solar distances, half the great axes of their orbits, yet this dependency in each of these groups is not to be maintained. We know for the present, as I had occasion to remark, of no inward necessity, of no mechanical law of Nature, which, like the beautiful law that connects the squares of the times of revolution and the cubes of the great axes, might represent the elements named in each group of planetary bodies as dependent from their solar distances.

If Mercury, the planet nearest the sun, is the most dense, six or eight times so as some of the farther ones, Jupiter, Saturn, Uranus, Neptune, the series proves very irregular in Venus, Earth, Mars, or in Jupiter, Saturn, and Uranus. The absolute magnitude increases, as Kepler already pointed out, with the distances in general, not in particular. Mars is smaller than the Earth, Uranus smaller than Saturn, Saturn smaller than Jupiter, and they are succeeded by a host of planets too small for exact measurement. The velocity of rotation generally increases with distance, but it is with Mars slower again than with the Earth, with Saturn slower than with Jupiter.

The world of bodies and forms is in the enumeration of its proportions to be represented as a fact, as existent and real, not as an object of intellectual conclusions, not of a connection discovered in its causes. No general law has been found here, neither for the celestial spaces, nor for the Earth in the site of culminating mountain-chains, or in the shape of continents. They are acts of Nature, born from the conflict of manifold conditions of projecting and attracting forces to us unknown. We step here with strained and unsatisfied curiosity in the unexplored empire of the becoming. —(Alexander von Humboldt.)

As socially we strive for legality, so we presume it everywhere in Nature. Kepler's sagacity remarked that strange chasm between the planetary distances. Lagrange proclaimed, with his authority as mathematician, that the density of planets increases with distance. Descartes had his theory of vortices; Newton's views told with characteristic reserve; Leibnitz, Franklin, and above all Laplace, formed their theory with more regard to Nature and Mechanics; Kant, Oken failed by profusion of ideas. Every philosopher, mathematician or not, high or low, ancient or modern, has meditated about order in Nature.

I have tried to trace these laws. I am no mathematician, not possessed with the analytical power of some to give to my thoughts that decisiveness of formulæ, often ill-founded, nor with the philosophical profundity of others; but refraining imagination, shaping my conclusions with the precaution of him who does not feel himself strong in the possession of knowledge, instrument, and reputation, I reach maybe at truth by logic.

The probability of a central sun, bright or dark, has been suggested; it is an explanation too eager for explaining. I deny this proposition. Indifferent how far we may recede with sun round sun, a central sun contradicts our concept of the immeasurable; it is a beginning, a fixed point from which Archimedes might lift creation from its hinges; it would be a centre, in the infinite there is none. The idea of the infinite and eternal is necessary, therefore true; there is something of which we must confess, with Schiller, "What the mind of the mindful cannot see, a childlike heart in its simplicity does find." If we examine only what is nearer, we do not know where we arrive, but we advance.

We shall not meditate if our sun revolves round another sun, it cannot escape existing influences; if it migrates from or to other systems which be the path of the individual between individuals. It has not always been what it is; it

had its process of generation and growth, like man and worm; but the act of engendering—of something out of nothing—touches on what we cannot comprehend.

We see the nebulæ of the heavens; they are resolved into stars by the telescope. "Telescopes and microscopes sometimes confuse pure reason; one is contented to find grubs," says Goethe. On earth there also are nebulæ; they condense to drops; a similar process exists in space,—we see it pass. An atom, we are told, is the beginning of man, two excentric atoms perchance; why not of sun and planets? sucking nutrition from a fog growing round them, becoming something from nothing.

Two central oppositions at least this nebula must contain; they are unequal, for they sprung from an inequality; they revolve more or less regularly,—revolving they advance; they received movement as they received existence; they are through that which was; without movement they cease.

We will rest in this mystic, yet true, circle, out of which we cannot step; we have only to inform ourselves about those ordinances of gravity which must connect and rule, in those relations of masses, distances, densities, velocities, inclinations, and excentricities. Manifold contradictions seem to mark themselves under comparative examination of our planetary system.

I do not pretend that what I find is always correct and complete in all its details, but I am convinced that it is true in principle and method.

To meet criticism beforehand I mention the series of planetary distances by Titius (Bode's law), derived from Kepler; it is an accord falsely taken in what there is, without theoretical foundation and explanation, but it attracts our attention. It has been said that the series does not entirely agree with reality. That should be the last objection. Where is there a law without cause of disturbance in itself? How many irregularities have been discovered in the motion of heavenly bodies, and, because nevertheless the law was believed in, one sought after reasons for these irregularities and often found them; sometimes not yet found them, sometimes pretended to have found them. If we have reason to believe in a principle, apparent contradictions shall not induce us to reject it forthwith, but to trace those contradictions to their sources, and our principle may come forth stronger than before from doubt and contest.

I shall at first make some concessions to probability, but we are taught to consider it as a law. Observation gives the

degree of probability; do we succeed to foretell events, probability increases to certainty.

Since a comet was seen to divide, such possibility cannot be doubted. I have demonstrated which shape rotation gives to a spheroid. Let our Earth be carried nearer the Sun by great excentricity of orbit, this new excentricity must be connected with a process of dissolution and condensation different from the actual one. The distribution of surface in continents and seas is disturbed; the earth alters the excentricity of its shape. As it gets nearer the sun, pressure, polar-ward undulation, condensation increases; dissolution is likewise augmented, and with it the effect of centrifugal force which carries matter to the equator. Thus a tendency to more rapid and to slower rotation is developed,—a tendency of the one part to make the slower rotation more rapid, and of the other part to make the more rapid rotation slower. In this action and reaction of different strata the interior condenses, the exterior dissolves, the fluid increases, the interior still possesses part of acquired velocity of rotation, the fluid part undercuts the solid with its small axis, as if an increased retarding sea suddenly poured under and uplifted Asia, America, the whole solid surface, which pressing forward were thus together, and divided cast off by the nucleus, the boiling vapours rushing between. The dissolved materials press each other, coagulate, surrounded by liquid and gas.

I should try in vain to follow the process of planetary rupture in a descriptive manner. The enumeration of simple effects of gravity, the development of some proportions, is not only more demonstrative, but also more comprehensive, than to introduce gravity in all its multifarious metamorphoses,—heat, magnetism, electricity, light,—which enliven the picture, attract imagination, but prejudice intelligibility, and carry belief and disbelief with them.

There exists in the planetary distances an interval which early and often attracted the attention of "conjectural astronomy." Examined more closely, indifferent if in consequence of an error in a star catalogue or of the intellectual motive of a fantastic hunt after law, it was found to embrace a host of planet dwarfs moving in intricate orbits.

As there can be no certainty, *à priori*, we shall put down, without pretension to accuracy, the farther limit of the zone of the asteroids at 680 solar radii from the sun. Likely that we do not know all planets, but those largest in mass, and therewith the whole mass approximatively. Including some to be found, making another supposition, I put down the

planetary mass at 1-680th of that of the sun; it amounts to the so-manyth part, as the sun is its own radii distant from the farther limit of that zone which is designated as "separating contracts."

The body exists by opposition, for by it density exists, which like all qualities is comparative. Suppose the whole planetary mass at this distance of 680 solar radii from the centre of gravity of the sun, without regard for the present that it must also extend away from the sun or this zone. Imagine the sun dissolved, reaching there, and both bodies of like density. Those two nebulæ with incipient nuclei, those vapour atmospheres, touch each other, flowing in one. There is no reason that their mean density should differ; their distance is proportional to their masses, divided by the square of distances, if we call distance the distance of the surface of the one from the centre of gravity of the other. The difference of density can only result from the variety of motions, and the development of this variety of motions we have to find.

The radii of these equally dense nebulæ with incipient nucleus (atom) are to each other as the cubic roots of their masses,—consequently the radius of the planetary mass is to that of the sun as 1 to 8·78, or roundly 8·8. Motion there must be; it depends on the relation of masses, and the original existence of motion decides the relation of masses: these are correlates; we have only to find what is and how it is.

This planetary mass revolves round the sun, because motion develops itself into regular rotation. With which velocity does it move? with the same mean one with which a planet would move there—about 54 Paris feet in the second. Translation is consequent on rotation; the converted sides repulse, the averted ones attract each other, as I proved. A point under the equator of this planet would then move in its rotation through about 54,000 Paris feet in a second, if such planet were of equal density with the sun moving at a distance of 680 solar radii. The rotatory action cannot be more rapid, only slower than the translatory one, than the quantity of motion obtained at all, and there is no reason that it should be less, as the densities of both bodies are equal, the distances their radii, the sun the larger one.

The velocity of motion is a gradual result of opposing masses. A planet thought possible to glide on the surface of the actual sun would move through 1,400,000 Paris feet in a second; consequently on the surface of that

planetary body of 1·680 mass of equal density through

$$\frac{1{,}400{,}000' \surd(\surd^3 680)}{\surd 680} = \frac{1{,}400{,}000' \surd 8{\cdot}8}{8{\cdot}8 \times \surd(8{\cdot}8)} = 160{,}000 \text{ feet.}$$

But the planetary mass did not have this density at the time; the solar mass could, notwithstanding its 680^3 times inferior density, produce a velocity of 54,000 feet next to its surface, for its centre of gravity was at a very little different distance then, as now, from the point of motion. The planetary mass also was 680^3 times less dense; its radius was not 1·8·8 of the present, but of the then existing solar radius; it measured 77·3 of the actual solar radii. The velocity which this mass was able to give to a mass at its surface was—

$$\frac{160{,}000'}{\surd 680} = \frac{54{,}000'}{8{\cdot}8} = 6200 \text{ feet}$$

in a second, which is still pretty near the velocity of a point under the equator of the sun in the revolution round its axis. The time of a whole revolution then was so much slower as the radius was longer; as the sun gradually attained to its present density the time of one revolution decreased; the distance run through in a certain time by a point of the surface remained the same.

Founding our structure on some remarkable but indefinite premises, we obtained conclusions which prove that the rotary velocity of the sun is nothing accidental, and that the planetary one is only altered by division of mass. We have gained the starting-point of the process. We shall consider if further deductions strengthen the probability that our supposition was correct.

Which was the centrifugal force on the surface and at the equator of the sun? Not as now—

$$\frac{1}{77{\cdot}3 \times 680}, \text{ but } \frac{1}{77{\cdot}3},$$

and which at the surface of the planetary mass is—

$$\frac{77{\cdot}3}{1},$$

which means it became under the equator 77·3 times greater than gravity.

By the thus resulting continuous separatory thrust the planetary mass could not simply dissolve, but it had to exercise its effect against the sun and away from it, as the

sun, too, affected by its motion the planetary mass; there was down and up undulation, a process which formed the central part of the planet in a revolving evolution cycloid,—that is the most descriptive expression,—and gave it besides conical elongations, polar axes, points of concentration, polar regions.

I call the solar radius R, the actual one r, $R = 680\,r$. The planetary distances in r are, r being the natural foot-rule which we must apply, $r = 96{,}350$ geographical miles (fifteen to a degree).

Mercury ...	83·6 r	Jupiter ...	1117 r	Uranus ...	4012 r
Venus ...	156 „	Saturn ...	2047 „	Neptune...	6447 „
Earth ...	214 „			A	9050 „
Mars ...	326 „			B	16560 „

The centrifugal force of the planet exceeded attraction 77·3 times; it repulsed the solar mass, which is 680 times larger.

$$\frac{77{\cdot}3}{680} = \frac{1}{8{\cdot}8}\ R = 77{\cdot}3r,$$

slightly less than the distance of Mercury. The centrifugal force of the sun was 1-77·3 we should conclude; it repulsed the planetary mass, which is 1-680 of the solar mass,—

$$\frac{680}{77{\cdot}3} = 8{\cdot}8$$

of the then planetary radii, consequently to 1360 r distance. But we have to deal not with a point, but with a body on whose dimensions the applied force produces unequal effects,—moreover, with a body whose centrifugal force exceeds its gravity, which, as it recoils, melts under way, which at the same time continually revolves, throws its mass forward and backward, and leaves it behind as itself moves off, which stretches itself, which with its extension alters the translatory and rotary velocity of its strata, which rebounds from the sun but at the same time is re-attracted towards it, which is an elastic body from which the sun concentrating recedes, as the planet before the sun. Step by step we must go with this column of matter, whose axis of rotation measures only 1-17·6 of its axis of central extension.

(To be continued.)

III. ON ARTIFICIAL LIGHT.

By An Old Technologist.

IF we compare the present times with the past we find in no respect a more utter contrast than in artificial light. There is firstly a great change in its quantity. In the proudest days of classical antiquity, just as amidst the barbarism of the Middle Ages, people of all sorts and conditions appear to have utilised the natural light of day to a much greater extent than do we. Noon, the culminating point of the sun, was approximately the middle of men's working and waking hours. Little or no daylight was spent in sleep, and, conversely, but little of the hours of darkness was devoted to work or amusement, or any kind of activity. It is quite outside of my business to trace the gradual changes which have brought about an entire divorce between the conventional and the astronomical meaning of the terms morning and evening. I may, however, briefly glance at the principal causes which have led to this social revolution. Foremost must rank the spread of civilisation, or rather of luxury, to countries situate under a high latitude and having a cold climate.

In the regions bordering on the Mediterranean the daylight, even in winter, lasts long enough for the inhabitants to get through a reasonable sufficiency of work. Early rising, too, in all but exceptional seasons, is there pleasant. With us the daylight during at least four months of the year is insufficient for the purposes of business. The mornings, too, except in summer, are damp and raw, and only men of exceptional vigour are found rising early from choice. The habits of late rising and of working by artificial light having once been formed, these practices have gone on extending.

Another reason for turning night into day is the greed connected with the organisation of modern society, which forces almost every man to devote more and more of his time, as well as of his energies, to the mere earning of a livelihood. The day is not considered long enough for work, and the only time left to the many for recreation, or even for thought, is the night. Hence we need wonder little that England, as the focus of industrial greed,* should be the

* Other nations are fast following our example.

most given to late hours. It is singular that among the points which the advocates of thrift systematically overlook the excessive outlay for artificial light should be included. Would we, or could we, once more make noon the middle of the day we should compass an annual economy of some millions sterling.

As regards the modes of production, artificial light has undergone changes no less striking than those which have affected its quantity, and to these it may be useful to advert, in view of the still further revolution which is, so to speak, at our very doors. A hundred years ago the art of lighting up private houses, public buildings, and streets had made but doubtful progress since the days of the Cæsars. In the majority of houses a single candle, made of soft tallow and requiring frequent snuffing, was the only light in a sitting-room. People in good circumstances used the "mould," made of a purified tallow and having a better appearance, though not giving a more powerful light, and needing the use of the snuffers. To have two such candles on the table was thought, even in the first quarter of the present century, a proof either of wealth or of extravagance. Among persons of high station wax candles were in use on especial occasions, and were carefully extinguished when no longer needed.

Lamps were never in former days as much used in England as on the Continent. They were generally confined to workshops, warehouses, passages, stables, &c., where a candle might be extinguished or made to "run" by the force of the wind. In such localities they were commonly, like the street-lamps, fed with whale- or seal-oil, and gave out a very unpleasant smell.

An important part in open-air lighting was played by torches or links, objects which have now disappeared from daily—or rather nightly—life, but which undergo a temporary resurrection in times of dense fog. As a curious survival may be noticed the gigantic extinguishers still to be seen attached to the front gates of some mansions in London. Into these, when the occupants of the carriage had alighted, the footmen thrust their torches.

The first great change in artificial light was the common introduction of gas. This invention, after having survived alike the usual opposition which every novelty has to encounter and the somewhat exaggerated praises of its admirers, has been found lacking in not a few points of importance. We are told, with technical truth, that gas is cheaper as a source of light than oil, tallow, &c. But there

is here a practical fallacy. The light, *e.g.*, of a candle or a lamp is thrown full upon the objects to be illuminated, leaving the rest of the room comparatively obscure. Gas, except when conveyed by means of a flexible pipe to a movable table-burner, is burnt in the upper part of the room, and illuminates the ceiling and the walls quite as much as the books, papers, &c., on the table for which it is chiefly wanted. It is doubtless true that for $1\frac{1}{4}d.$ we may obtain as much light with gas as we could with colza oil burnt in a solar lamp for $2\frac{3}{4}d.$ But if, as is generally the case, we have to produce at least three times as much light with gas as with an oil-lamp, in order to read or work with equal ease, the economy disappears. I think the experience of every man who has introduced gas into his house will be that he is out of pocket by the change. He obtains more light than he did from lamps, but not where it is most wanted.

Another defect of gas as a source of light is its unsteadiness. Theoretically it may be, perhaps, possible to make gas burn with a perfectly steady flame; practically speaking this desideratum is not reached. If the flame of a gas-jet, of whatever construction, is carefully watched it will be found constantly flickering. The effects of this unsteadiness are, in the long run, exceedingly irritating to the eye.

A third, and perhaps more important, shortcoming lies in the products of the combustion of gas, which are of course emitted into the atmosphere of the room. Among these must rank sulphurous acid, which in contact with the moisture of the air passes gradually into sulphuric acid, and has an injurious action upon pictures, wall-paper, the bindings of books, &c. Ethylene is probably never entirely absent among the products of the combustion of gas. The amount of watery vapour and carbonic acid is also excessive. Hence, except there is special provision for ventilation at the level of the ceiling, the atmosphere of a room lighted brilliantly with gas becomes most oppressive, and persons much exposed to it suffer from headache and languor. This is the case with visitors to our theatres, few indeed of which are even tolerably ventilated. Still worse is it the case with the assistants of certain drapers, &c., who are obliged to work by artificial light every evening save Sunday from 4 to 9, 10, and in some cases even 11 p.m. The nature of the atmosphere in these shops may be guessed from the quantity of moisture which may be seen runnning down the insides of the window-panes on a cold evening. The carbonic acid given off must reach a correspondingly abnormal proportion.

A further disadvantage is the quantity of heat generated by the use of gas as an illuminant. I will admit that there are in our climate very few weeks in the year in which heat, *per se*, can be regarded as a nuisance. But heat, like light, may be produced and applied in the wrong place. If we consider the tendency of heated air to rise upwards, we shall see that the sources of heat should be placed as low as possible. In a room lighted with gas this arrangement is reversed. Up aloft is, so to speak, a stagnant lake of hot air in which the heads of the inmates are plunged, and are further heated by radiation from the ceiling and the upper parts of the walls. At the bottom the air is cold. I have observed a difference of 9° F. between the temperatures of such a room near the ceiling and near the floor respectively, and I have reason to believe that there are cases where a much greater difference would be registered. If there is any trust to be put in the old maxim, to keep the feet warm and the head cool, this is a condition the very reverse of salutary, and experience fully confirms the supposition.

I know it is contended that, for equal amounts of light, the heat generated by oils, tallow, stearine, wax, paraffin, &c., is greater than that thrown off by gas. This is true, but not to the purpose. The heat coming from lamps and candles is developed at a lower point in the room, so that there is a less extreme difference between the ceiling-temperature and the floor-temperature. Again, as I have already mentioned with reference to the question of cost where oils or tallow are used, a less quantity of light is demanded.

Another defect of gas is so well known that we may feel surprise at the little attention which it practically receives. Gas is poisonous if inhaled even very largely diluted with air; and such mixtures, further, according to their proportions, are more or less powerfully explosive. Hence in premises lighted with gas there is always a certain margin of danger which does not exist where candles or non-volatile oils are used to give light. A slight defect in the work of the plumber or of the manufacturer of gas-fittings,—neither of whom is immaculate,—or a casual oversight on the part of a child or a servant, may lead to a serious calamity. Even if a man can guarantee the utter absence of carelessness within his own doors, or if he eschews gas-lighting altogether, he is still at the mercy of accidents as long as it is used by his neighbours or by the local authorities. Many of my readers will remember the explosion which began in Charlotte Street, Fitzroy Square, and which extended—

tearing up streets and wrecking houses—even to the east side of Tottenham Court Road.

I may therefore sum up the defects of gas-light as including unsteadiness of flame, emission of products of combustion harmful to persons and property, production of high temperature in upper parts of rooms, and danger of leakage or escape, whilst at the same time the commonly alleged economy of gas is in practice doubtful, or at most very slender.

Hence it is evident that coal-gas as a source of light is very far from perfect. It appears, also, that its defects are of a nature not to be readily overcome or avoided by any improvement in its manufacture or mode of supply and consumption. It is scarcely conceivable that gas can be purified in such a manner as to be absolutely free from sulphur. Still less can it be rendered non-explosive when mixed with common air or compelled to burn with the absolute steadiness of a well-constructed lamp.

I turn now to another great step in the production of artificial light—the introduction of the mineral oils. These oils, when burnt in proper lamps, have certain advantages as well as defects. They are much more economical than refined vegetable oils, and give a brighter light than coarser seed or fish oils. They are probably in practice cheaper than gas, since they produce the light where it is wanted, and not in the upper part of the room. They are free from sulphur, and hence do not injure textile goods, books, furniture, pictures, &c. Otherwise their products of combustion are little different from those of gas. Being generally placed on tables, &c., they do not overheat the upper part of the room.

But to balance these good properties come defects which cannot be ignored. Like all lamps, they are much more troublesome than candles or gas-jets. They must be cleaned, trimmed, and supplied with oil by daylight, and can never be safely replenished whilst burning. If inferior qualities of oil are used explosions of a serious nature may happen. The glass chimneys are liable to break if imperfectly annealed, or damp, or if the flame is turned up too suddenly. The overturning of a lamp—a not impossible occurrence—may easily give rise to a conflagration. Nor can such lamps be safely turned down if for the time being a good light is superfluous, for when burning dimly the products of their combustion are exceedingly unwholesome. To sleep with a petroleum lamp in the bed-room, turned down to a mere speck of light, may prove fatal. Hence mineral oil, though

it is of immense value and utility, is still not all that we could wish for.

There is, therefore, room for the improved system of lighting which is just being introduced—the electric light, in its two forms of the "arc" and the "glow" lights.

Being neither an inventor in this department nor a shareholder or official in any electric light company, I may, it is to be hoped, be recognised as impartial when I pronounce this system to be "the light of the future."

Its advantages over gas and oil lamps are the following:—There is an absence of products of combustion mingling with and deteriorating the atmosphere. Most of the electric lights are enclosed in hermetically sealed globes. There is no consumption of oxygen, no escape of sulphurous or carbonic acid, ethylene, or other product of combustion. The ceilings are not blackened; books, &c., are not corroded, and respiration is not interfered with. Practically no heat is liberated, so that the upper part of the room is not hotter than the floor. The feeling in such a room, however brilliantly lighted, is the same as in the daytime.

The colour of the electric light, though not absolutely identical with that of sunlight, makes a nearer approach than that of any artificial light hitherto used. With the arc light colours can be judged and compared as in the daytime—a property which during the short dull days of winter will be of great use in dye-, print-, and colour-works.

A notion prevails that the arc light is more blue than daylight. This is, according to the best authorities, an error, since if the two are actually compared the arc light is decidedly the yellower of the two. But being much less yellow than gaslight, or than other artificial lights with which it might come in contrast, it seems slightly bluish. Hence it is in some quarters evil-spoken of, because it shows up artificial complexions and textile goods of inferior colours. I have heard that certain drapers of the modern encroaching school, who had introduced this light into their shops, have found it disadvantageous, as revealing unpleasant truths.

The glow light or incandescence light approaches gas more closely in its colour, and hence may be preferred by those whose eyes have got accustomed to a yellow light.

The expense of the electric light varies very considerably according to the locality. Where a constant source of moving power exists near at hand, such as a stream of water, light may be obtained for merely the interest on the first cost, the depreciation and the carbons consumed,—in all a mere fraction of the expense of a much inferior quantity of

light obtained with gas. Mr. W. Crookes, F.R.S., has already laid before the public the comparative cost of gas and of the electric light (incandescence) as applied in his house, the advantage being in favour of the electric light, though produced under disadvantageous circumstances; that is to say, the motive power had to be produced solely for the purpose with a "silent gas-engine,"—certainly not the most economical method of obtaining mechanical power, though the only one available in such a locality.

In an experiment in street-lighting carried out by the firm of Siemens and Halske, the eminent electrical engineers of Berlin, the cost of lighting up a given area at an equal intensity with the electric light was found about half that of gas.

As here also the motive power was not obtained in the most economical manner, there can be no doubt that the general introduction of the electric light will represent a considerable saving in money, over and above its other advantages.

IV. SUN-SPOTTERY.

IN MEMORIAM OF THE LATE PROFESSOR STANLEY JEVONS.

By A. H. Swinton.

PERHAPS in no way has Science exerted a more beneficial influence on society than by the innovations it has made during the nineteenth century on the old astrological notions of our ancestors. As far back as the seventeenth century the discovery of the revolution of the globe showed that their ideas of aberrant planetary motions were mere optical illusions,* and paved the way to a recognition of the sun as the true centre of the universe. It was but a step then to attribute changes in terrestrial phenomena to corresponding changes transpiring in its light-giving

* The question whether the position of the planets is in relation to the sun-spots has been discussed by Dr. Muirhead (Phil. Soc. Glasgow, vols. for 1875, 1880) and Mr. F. B. Edwards (Nature for 1882, p. 293).

photosphere, and it must remain a standing marvel that philosophers should have remained so long without seizing the proper clue to unravel the many mysteries that surround us. Nor is the observation of spots on the sun's photosphere any new thing, There is the ancient Chinese record purporting to refer to these very spots, and various scattered notices coming to us from the Middle Ages. But these records, if they possess any precision, are involved, and apparently unsuitable to convey a just conception of the solar cycle: a glance over them reveals at once that any system deduced from them is not in accordance with the normal eleven-year periodicity of the sun-spots,—in this case the only criterion. The application of the lens to the telescope in Galileo's time caused the sun-spots to become a subject of more exact scrutiny, and from this period until the days of the elder Herschel they became a subject of comment in astronomical circles, and, as might appear, did not wholly escape the notice of the fair sex, who patched their faces to imitate the sun. To Herschel belongs the credit of propounding the question whether these solar changes did not affect the recurrent seasons, and consequently influence the corn harvest in our planet; but his investigations of this subject would appear to have been somewhat premature. The labours of the indefatigable Schwabe, in daily chronicling the number of spots on the sun's face, have more lately been supplemented by photographic observations, and the result is that these spots have been observed to come and go, their maximum of prevalence being attained normally every eleven years.

It is important, then, to inquire if there be any natural phenomena that reappear on our planet in cycles of eleven years which may be referred to the influence of these sun-spots. We find, for example, that the time of sun-spots has been shown to coincide with a period of cyclones and hurricanes, a period of greatest rainfall, and a period of electric storms; and, on the other hand, that the time of no sun-spots coincides with a period of deficient rainfall and tropical drought. All this is of the first commercial importance. When storms increase shipwrecks become numerous, and a general depression ensues in certain industries, while an augmentation of the rainfall decidedly means a deficient harvest in this country,—witness the cry of distress that arose in 1879 from our agricultural districts. When tropical drought arrives famines are prevalent, and heat and insects conspire to injure cargoes of imported grain; locusts make organised inroads on the standing corn lands. None

of this matter is surmise, but the conclusion of legitimate deduction, arrived at by strict observation and computation on the part of the leading meteorologists of the day. On the other hand, electric storms are calculated to disturb the recent enterprise in electro-magnetic cables, lighting, and locomotion, that has so fair a promise, while the question of spots or no spots will be brought to our very homes in the electric light.

That other phenomena that affect the well-being of mankind may safely be attributed to the sun, the following fragment of a chronicle of the eruptions of Vesuvius and Etna, the result of some considerable research, will demonstrate: —In 1766 sun-spots were at a minimum, and Etna was in eruption, and the next year Vesuvius. In 1769 sun-spots were at a maximum, and Vesuvius erupted during the space of three years. In 1775 sun-spots were at a minimum, and an eruption of Vesuvius that commenced in 1773 terminated in 1776. In 1778 sun-spots were at a maximum, and there was an eruption of Vesuvius for two years, followed by an eruption of Etna for two years. In 1784 sun-spots were at a minimum. The previous year Vesuvius and Etna were both in eruption, the activity of Vesuvius lasting for three years. In 1788 sun-spots were at a maximum, and the previous year Vesuvius and Etna were both in eruption, the period of activity lasting three or eight years. In 1798 sun-spots were at a minimum, and Etna was in eruption for about five years. In 1804 sun-spots were at a maximum, and Vesuvius was in eruption. The succeeding year both Vesuvius and Etna were active. In 1810 sun-spots were at a minimum, and the previous year both Vesuvius and Etna were active, the activity of Vesuvius continuing for two years, and being followed by an outbreak on the part of Etna. In 1816 sun-spots were at a maximum, and Vesuvius was in eruption during two years, and afterwards Etna was in eruption during the space of one year. In 1823 sun-spots were at a minimum, and Vesuvius had been erupting during two years. In 1828 sun-spots were at a maximum, and Vesuvius was active. In 1833 sun-spots were at a minimum, and there was a corresponding activity for six years on the part of Vesuvius, while Etna was in activity the year previous. In 1838 sun-spots were at a maximum, and Vesuvius and Etna were both active. In 1842 they were at a minimum, and Etna was active. In 1847 sun-spots were at a maximum, and there had been eruptions of Vesuvius for three years previous; the first year Etna was also erupting. This period was succeeded by a slight eruption on the part

of Etna. In 1855 sun-spots were at a minimum, and there was an activity on the part of Vesuvius for four years. In 1860 sun-spots were at a maximum, and Vesuvius was active during the space of two years. In 1867 sun-spots were at a minimum; Etna was in eruption two years previously, and Vesuvius the two subsequent years; the latter year Etna was again erupting. In 1870 sun-spots were at a maximum, and there was an eruption of Vesuvius extending over three years. In 1876 sun spots were at a minimum; Vesuvius was in activity, and there had been eruptions from Etna during the space of three years.

I have thus adduced sufficient to render it palpable that Solar Physics, in its concrete form a matter of astronomical observation, is capable of being turned to utility when applied to establish statistics that are in legitimate connection. It is, however, evident that these statistics are general in their enunciation, and that they may or they may not affect pecuniary interests or produce commercial panics. However let us look at the economic matter in the same light as the natural phenomena. Supposing the said pecuniary gains and losses, and the said commercial panics, should occur in cycles, and that these cycles should prove to coincide with the sun-spot cycle. What then? Must we at once fly to the assumption that, although the periodicity is identical, there is a similarity and not identity of causation. Let us take the well-known case of contradiction presented by the harvest and crops in India. Messrs. Norman-Lockyer and Hunter have constructed a Table which clearly shows how the famine years in Southern India coincide with the years of fewest sun-spots when the rainfall is deficient; the late Prof. Stanley Jevons has constructed a table that shows that corn was at its highest quotation, in the Delhi market, at times when sun spots were normally at a maximum and the rainfall in excess. What then? Is there here anything but what might be supposed to result from a difference of latitude or surroundings? May not rain that refreshes a parched land arrive with different auspices in a more humid climate? Yet who is the individual required to solve this problem, the meteorologist or astronomer? Decidedly not; it belongs to the speciality of the naturalist.

Why should we not, then, have an efficient staff of observant naturalists as adjunct to each Solar Committee of the future? In vain the men of figures will puzzle when it appertains not to their sphere to investigate. In vain red-tape clerks and smart newspaper reporters will raise a cry of sun-spottery! sun-spottery! when the oracles are at fault.

Let us suppose a case in point. Suppose the quotations of corn in this country do not constitute a cycle at harmony with the seasons and sun-spots, what are the unknown factors? Astronomers, clerks, and reporters grow confused alike; some talk of Providence, others believe in Zadkiel's Almanack. Well, here certainly are some of the desiderata. It has always appeared to me a desideratum that merchants in grain should keep strict record of weeviled cargoes and weeviled granaries; as merchants, by registering the profit and loss, and, if also entomologists, by noting and estimating the increase and diminution of the noxious species of beetle. It has always appeared to me a desideratum that farmers should note when the chaff falls from the threshing red with the maggots of the wheat midge; that they should chronicle the growth of charlock, poppies, and other tares that carpet their fields; that loss from smut, mildew, and rot should become a matter of exact register. These agencies are all injurious to corn, and their virus will vary as the average dryness or humidity of the year; then, over and above, there remains the question of export and import. When all these little trivialities are understood, and not till then, shall we be fairly in a position to clench the problem of Solar Physics applied to the monetary statistics that so absorb our attention.

But it may be asked, Do the corn quotations in this country never constitute themselves into a cycle, moving in consonance with the years and sun-spots. Let us awake to the broader prospect furnished by the past history of trade and science. It is the rain that causes the green herb to grow for the service of man; it is the rain that renews the face of the earth and paints the vernal meadow with leaves and flowers. In this humid climate, however, an excess of rainfall is a detriment to the harvest-field in the opinion of every well-informed agriculturist. Especially does an excess of rain mar the promise of the wheat, the wheat plant which replenishes the marts of the Mediterranean and the United States of America being much less truly acclimatised with us than its bearded sisters the barley and oats, that conserve their wiry and amazonian natures despite of culture. The rain, again, which is here the fostering or malignant influence, is itself but dropping vapour distilled by the superior agency of the sun from off the water surfaces that cover the earth, and the said vapour or cloud is distributed to its allotted place by the winds. In the case of both rain and wind heat is the motive power, and any increase or diminution in the heat of the sun must depend on changes in the potency

of its photosphere, and such changes as we believe to be indicated in the appearance and disappearance of the solar spots.

Of late years the rainfall of the British Islands has become an object of special study and investigation, with amateurs as well as paid officials. Mr. J. J. Symons, of the Meteorological Society, who has recently published statistics gathered from numerous quarters, takes occasion to refer to the existing caprice of heaping ridicule on the astronomical precedent of referring the phenomena of Nature to fixed and pre-determinate laws. He calls attention to the circumstance that the wettest years in England of late prove to have been 1836, 1841, 1848, 1852, and 1860, and that the first, third, and fifth of these years fall into a twelve-year period. He further calls attention to the circumstance that the driest years in England of late prove to have been 1826, 1834, 1844, 1854-5, 1858, and 1864, and that the second, third, fourth, and seventh of these fall into a ten-year period. Now it is at once manifest that the twelve-year period of wet years he here indicates coincides with the solar cycles, and that the dates only differ by two years on the whole from the epochs of most sun-spots assigned by Prof. Wolf. The ten-year period of dry years, which alternates with these wet years, will then consequently correspond to the minima years of solar spots, from which in three of the instances they differ but one year.

If there is indication that the sun-spots may be traced in the amount of English rainfall, the question then arises—How does our rainfall affect the growth and ripening of the wheat? Let us summarise the corn statistics. Mr. J. E. Thorold Rogers, some years back, undertook the arduous labour of tabulating the price of corn in England from the thirteenth down to the commencement of the seventeenth century, and has embodied the result of his researches in four goodly volumes. Going over the columns of his Tables, and counting eleven years for the return of the maxima of sun-spots, we observe an unmistakable rise in the price of wheat at every year the pencil checks,—from 1259, when the liberal-minded Henry III. became the victim of his turbulent barons, to 1582, the year when the calendar was rectified by Pope Gregory XIII. As a rule we notice that the disposition to rise precedes the years to which we assign an excess of solar spots; and this occurs with a regularity that might surprise those who, drawing from the depths of their inward conscientiousness, have hitherto failed to perceive the supremacy of our public light in mundane matters. These

cycles of high prices are marked by depressions for the most part well indicated, when, as would appear, the gracious goddess of the golden year has harnessed her dragons, and emptied her cornucopia on old Albion's shore. After a lapse of sixty-six years the musty Audit Books of Eton College furnish us with the prices of middling or mealing wheat per quarter, at Windsor Market, from 1646, the year after the defeat of Charles I. at the battle of Naseby, down to 1826, when George the Fourth was king. During all this long time there undulates a marked rise and fall in the wheat quotations, the crests of the waves corresponding as they rise to the epochs of most sun-spots determined by Prof. Wolf. Nor is there here any irregularity to be discerned, save that these crests have reached a climax sometimes a little before, and sometimes a little after the year specified; or, more rarely, they are a little bifurcated. From 1771, previous to the alteration of the Corn Laws, down to 1852, when they were repealed by Sir Robert Peel, we possess Tables of the average price of British corn per imperial quarter in England and Wales, as ascertained by the receiver of Corn Returns; and from 1859 down to the present time we possess the Parliamentary Reports or the Agricultural Returns for Great Britain. Throughout these long columns of quotations the same fluctuation of price in correspondence with the sun-spot years is easily checked off, the only peculiarity presented to the eye being that the monetary cycles exhibit a tendency to become somewhat less marked, owing to the increasing import of grain that began to take shape about the commencement of the eighteenth century.

The prices of barley and oats fluctuate as the wheat quotations, but in cycles less pronounced. Thus 1879 was a wet rainy summer over the corn lands in the South of England, and it brought with it great distress among the agricultural classes: wheat, however, actually fell in price, on account of the large importation from America at this time. On the other hand, 1878 and 1880 were years when the summer rainfall over the West of Scotland fell to a minimum, and they came with a rise in the price of *British oats*, according to the Excise Statistics. From this it would appear that oats suffer less from a wet season than wheat, and I believe the same is true of barley in less degree. As regards insect ravages, as late as 1876—which was both a locust year and a year of minimum sun-spots—the wheat crops of America, according to Government Statistics, gave the poorest yield per acre they have done these thirteen years past. Here are reasons, then, why the prices of grain should

not roll and break as the solar phases transpire; the machinery of Nature is ever complex,—we must know the disturbing causes and understand the conflicting tides. For the present it is sufficient to know that the sun-spots raise the waves. Perhaps the refusal of mother Earth to nourish her offspring awakes the passions of mankind. Will anyone disprove it,—we can all ridicule,—is famine a provocative to war? War influences commerce by checking the export and import of grain. War and famine conspire to produce a commercial crisis; there was one in 1857, and if you count the elevens backwards and forwards you may find more.

The days may come—and I do not despair of seeing them—when the sun-spots shall not only be a subject of investigation for the curious, but when they shall photograph themselves on the frontal of our social life, as of olden time the patches flecked the countenances of the scientific fair. Evidently a large class of economic phenomena, the subject of recurring comment in the daily papers, are under the influence of the sun-spots, and there is a growing tendency on the part of our men of Science to clinch the matter. The sun-spots are now supposed to affect our globe in an immediate manner, and to constitute a telegram that is immediately transmitted to the forces that rule our atmosphere. Take in illustration Prof. Balfour Stewart's late communication to "Nature," on what he has styled Magnetical and Meteorological Weather. As is known, the movements of the magnetic needle when rendered very sensitive are three—the secular, annual, and diurnal. The secular variation is due to the movement of the two points of greatest cold about either pole of the earth, which attract the needle, and by their motion draw it from east to west, and *vice versâ*, in about 236 years. In its yearly and daily variations the needle follows the sun flower, as one would infer from being repelled by the heat-wave that passes with the sun over the earth. Thus it is farthest from due north about the spring equinox, and nearest that direction at the summer solstice; at sunrise it progresses slowly westward, an hour or two after midday it turns and begins to go back, and about ten o'clock at night it regains its original position, remaining almost stationary until sunrise next morning. The daily movements of the needle are not only horizontal, but also vertical; and these movements, horizontal and vertical, increase annually as the sun becomes leperous with spots. When the sun is most spotted the oscillation of the needle increases, and one might surmise this to be directly due to the influence of the spots as they hourly form and vanish.

Now Prof. Balfour Stewart says, in the communication in question, "Sometimes we have very hot days and cold nights in which the diurnal temperature range is very great, and these are succeeded by close rainy weather in which the diurnal temperature oscillation is very small. In like manner we have sometimes a very large, and at other times a comparatively small, diurnal oscillation of the magnetic needle; so that it also is affected by the influence of a magnetic weather." He then shows, by a Table, that the diurnal oscillation or declination of the needle corresponds in this country to the weather or range of temperature that may be expected to arrive six or seven days afterwards. As a reason for this he adduces evidence that the temperature range weather, when once produced, travels from west to east, taking probably on an average eight or nine days to cross the Atlantic; and that there is also evidence to show that the declination range weather travels likewise from west to east, but quicker than the temperature range weather, taking about two days to cross the Atlantic.

The conjecture is not remote if we follow the reasoning here suggested, that the fine and dull weather we experience on this planet vacillates as the spots come and go on the sun's photosphere; and if the weather, may it not be that the storms do so likewise? Now at the moment of writing a circumstance has occurred that would certainly lend colour to such a conjecture. On the 18th of November, 1882, a huge spot was seen on the face of the sun; on the 22nd of the same month warning came from New York that a dangerous disturbance would arrive on the British and Norwegian coasts between the 24th and 26th, attended with gales, snow, and rain. As I am now writing, on the 27th of November, wind, snow, and rain are holding jubilee, and raving about the sun-spot. Have we here any true con nexion, or merely a coincidence?

V. FRESH NEWS ON EARTHWORMS.

IT is noteworthy how much we find is to be learnt concerning even the most despised plant or animal when once some genuine observer points the way. Darwin's researches on the earthworm had extended over forty years, yet the subject is not yet exhausted. There was room left for Mr. Kinahan to make valuable and interesting observations, which are recorded in our last volume.* MM. Pasteur and Feltz have shown that these little creatures may be a source of danger by bringing to the surface virulent matter from the bodies of buried men and animals. M. Trouessart has observed that a common species (*Lumbricus agricola*) constructs towers at the mouth of its burrow, just as is done, according to Darwin, by the nearly allied genus *Pinchæta*. And now we find that Prof. Victor Hensen, Herr von Lengerke, and the Danish zoologist P. E. Müller† have all been engaged with the study of earthworms, and have elicited additional facts in connection with their habits, their food, their part in the formation of "vegetable mould," as it is commonly called, and their influence on the fertility of the soil. This, as we shall see below, is probably even greater than was supposed.

It was formerly considered that humus was derived entirely from the decomposition of vegetable matter. Darwin, as we know, showed that animal agency played here an essential part, and concluded that all such mould had been passed again and again through the intestinal canal of worms, and owed its characteristic properties to this action.

As far back as 1865 H. Thiel, without reference to the earlier researches of Darwin commencing in 1837, observed that the roots of red clover penetrate into the soil to the depth of 2 yards and more, by means of tubes or channels pre-existing in the sub-soil. These channels he thought might have been formed by the roots of trees which had formerly traversed the soil and had subsequently decayed; or by some animal. What animal he does not appear to have ascertained.

In 1877 Prof. Hensen made a further advance. He seems

* Journal of Science 1882, p. 331, 377.

† *See* Landwithschaftliche Jahrbücher, 1882, p. 661; Der Norddeutsche Landwith, 1881, p. 568; and Biedermann's Central Blatt, 1882, p. 723.

to have been aware of Darwin's earlier memoirs on the subject, but not of the observations of H. Thiel. He observed that the channels by which the roots of plants penetrate into the subsoil are chiefly made by the large earthworm (*Lumbricus terrestris*), which at the same time lines them with finely divided mould.

Hensen finds that the large earthworm operates in the subsoil, whilst the smaller species (*L. communis* and others) work in the upper layer of the soil. All of them in the night thrust their bodies partially up out of their holes, and drag in decaying leaves for food. This author illustrates his work with two plates, showing the course of the worm-holes, the manner in which they are lined, and the roots by which they are traversed. The holes or tubes he finds most distinctly marked in sandy soils, where they descend vertically to a depth of 3, 4, and even 6 feet, and then extend further in a horizontal direction. The sides of these tubes are studded more or less closely with little black prominences, often of a line in diameter, which are the excrement of the worm, as appears on microscopical and chemical comparison with the excreta taken from the body of the worms.

In tubes which have existed for some time there are generally found roots of the plants which are growing on the surface. Such roots, of $\frac{1}{2}$ to $\frac{1}{4}$ line in thickness, run even to the very end of the tubes, giving off all the way minuter rootlets covered with capillaries which spread over the sides. This phenomenon, says Prof. Hensen, may be best observed with plants of the cabbage tribe. It will be readily admitted that such roots are placed under very favourable circumstances for growth and nutrition. The matter taken from the sides of the tubes, and that from the intestines of the worms, differs little in its composition from leaf-mould.

From his analysis Hensen concludes that worms certainly eat soil, but only in order to make their tubes. Their real nutriment consists of half-decayed vegetable, and incidentally animal matter. Ordinary garden mould would scarcely afford them a sufficient quantity of assimilable matter, though leaf-mould might do so when not too far decayeed.

As regards the fertility of a soil the earthworm exerts a favourable action from several points of view:—

1. It insures an equable distribution of leaves and other loose portions of organic matter, and removes them from the action of the wind.
2. It promotes the decomposition of such organic matter by comminution.

3. It distributes the fertilising matter in the subsoil, and renders it more accessible to the uniform action of air and moisture, thus aiding in the liberation of the mineral constituents of plant-food.

It is well known that soils, and especially subsoils, may contain a large proportion of the elements necessary for the nutrition of plants, but in such a state as to be practically unavailable. To render them assimilable they have to be decomposed by the action of the carbonic acid contained in the atmosphere, and by water. These agents are admitted into the soil by the mechanical operations of agriculture, and probably even to a greater extent by the unceasing work of the earthworms. Hence Prof. Hensen makes the startling —but we believe perfectly justifiable—assertion, that chemical analysis of a subsoil gives no sufficient insight into its fertility if the number of worm-holes are not also taken into due consideration.

As regards the quantity of earthworms living and boring in the soil, we do not find any absolute agreement among different authorities. This is merely what might be expected from the differences of soil, climate, season, and other conditions. Hensen calculates per acre of arable soil 110 lbs. of worms, weighed with their intestines emptied of soil. H. von Lengerke takes a considerably higher estimate. In an acre of meadow-land he found 176 lbs. of *L. terrestris* and 550 lbs. of *L. communis*, or in all 720 lbs. of worms. In strange contrast with these figures would be the number discoverable in suburban gardens around London, which in barrenness are probably unsurpassed.

Prof. P. E. Müller has studied the behaviour of worms in woodland soils, especially in the beech-woods, in which Denmark is—or perhaps rather was—so rich. The beech tree flourishes in the mild beech-humus, but dies off on the beech-moors,—that is, as we understand the author, on soils where beech-woods have once existed. In the former class of soils earthworms live in plenty, whilst in the "beech-moors" they are entirely wanting, and indeed animal life is altogether very sparingly represented.

On mossy, heathery, and boggy soils, worms are not numerous in most parts of England. According to Mr. Kinahan the same rule holds good in Ireland.

Like Prof. Hensen, but unlike Darwin, P. E. Müller is of opinion that the earthworm is not nourished by the soil. It eats the soil, indeed, but as a method of opening its tubes and removing the excavated material. Any

vegetable matter thus swallowed will of course be utilised as nourishment.

As regards the horizontal subsidence of stones, rocks, &c., Müller does not, like Darwin, find the phenomenon satisfactorily accounted for by the removal of the earth from beneath by worms, and its gradual accumulation around their sides. Here his observations are completely in accord with those of Mr. Kinahan as laid down in our last year's volume (p. 377). It does indeed seem that in this respect Darwin has somewhat overlooked other agencies. To one of these, so familiar as to be forgotten, we must beg for a moment to direct attention. Everyone must have noticed how on a frosty morning—especially if there has been previously much wet weather—pebbles, fragments of stone, &c., which have been imbedded in the ground, appear to have sunk down below the surface. This is to be noticed even in compact gravel-walks where worms are entirely absent. The reason of this is plain: the water held by capillary attraction within the pores of the stone expands on freezing, and thrusts the loose soil upwards. When a thaw succeeds, the soil or gravel falls back to some extent into its old position, but not entirely so; the edges of the stone after each such alternation are more and more covered, until at last it disappears from sight. Something of the same nature will take place, to a slight degree.

Another agency which must probably contribute to the subsidence of stones, and even to their translatory movement on the slope of a hill, must be sought in the expansions and contractions which are known to take place in the earth's crust in accordance with changes of temperature and barometric pressure, and in those slight vibratory movements which are far from uncommon. It is well known that if we mix together in a box fragments of stone, &c., with sand or other similar powder, and set the box aside in what we think a quiet place, in the lapse of years these different materials will sort themselves out, the larger fragments finding their way to the bottom. This process is constantly going on in Nature, as far at least as regards stones resting on arable soil, loose sands, gravels, and peat earth. Mr. Kinahan, in the article referred to, makes mention of a number of other agencies which are undeniably concerned in the result. Hence without seeking to deny that worms take a part, and occasionally a prominent part, in the subsidence and displacement of stones, we submit that a complication of causes is here concerned.

P. E. Müller combats an objection which has been brought forward,—to wit, that worms cannot effect very great changes in the subsoil. As Darwin admits, they live chiefly in the upper stratum of the soil, and only retire lower down in severe cold or in prolonged dry weather. To this Müller replies that the smaller earthworm (*L. communis*) frequents the upper layer of soil, in which it is not unfrequently frozen up in a hard winter; but that the larger species, as is maintained also by Hensen, works in the subsoil, and rarely visits the surface except in mild rainy nights.

The importance of worm-tubes in aiding the penetration of the roots of plants into the soil has also been called in question, chiefly on the faith of observations which are not quite to the point. Suppose we fill a pot with earth or sand, quite free from worms, and introduce a plant; its roots will quickly make their way downwards. But such soil or sand is comparatively loose and porous. In the fields and gardens the soil, even where it is within the action of culture, is relatively compact, and below the reach of the plough or the spade it is very dense indeed, almost impenetrable to roots. Thus the above-mentioned experiment is illusory. It is also proved by direct observation that the existing tubes or channels are preferentially utilised by the roots of plants. Thiel, to whose researches we have referred above, admits that the roots chiefly penetrate down into the substratum by means of such channels, but he doubts whether they derive any important share of nutrition from the subsoil, as they do not touch the sides of the tubes very closely, but often seem to hang in them loosely suspended.

But Müller shows, in reply, that the minute capillary rootlets are most active in the absorption of food-matters, and these are particularly abundant in the tubes in the subsoil, where they meet with favourable conditions for growth, such as an equable temperature, an unexhausted soil, a moist air, rich in carbonic acid and space for undisturbed growth. The capillaries not merely spread themselves over the sides of the tubes, but make their way into minute chinks and apertures.

Another important feature is here brought prominently forward by Prof. Müller—the underground dew. In the severe winter of 1879-80 he made the following observation:—After a frost of several weeks uninterrupted duration the frozen stratum was dug through. Immediately below it was a moist subsoil of from 1 to 2 feet in thickness, but on penetrating still lower down the earth was again drier. The presence of the moisture can be accounted for only on

the supposition that the watery vapour constantly rising up from the lower strata became condensed as it approached the frozen upper layer, the immediate subsoil being thus kept constantly moist. In the worm-holes the roots thus constantly enjoyed a moist atmosphere. Prof. Müller, indeed, found them frequently covered with drops of water, and is thus led to suppose that there is in these tubes a practically incessant formation of dew. The condensed water coming in contact with the sides of the tubes will dissolve both saline and gaseous matters which are then absorbed and assimilated by the roots, which are thus supplied with nourishment as well in drought as in moist weather.

We generally find nothing in Nature which is altogether hurtful or altogether beneficial. The old and profoundly wise proverb "It is an ill wind which blows no one any good" might well have its converse, since the most beneficent and necessary agencies have their evil phase. Seeing, therefore, what an amount of benefit we receive from worms, we may well ask to see the other side of the account. Singularly enough this other side is almost wanting. It is sometimes asserted that worms feed upon the roots of living plants. This is, however, an error; they consume the roots of dead and decaying vegetation only. We may, indeed, find the earthworm active amongst rotting bulbs and tubers, but in such cases he is not the cause of the mischief. He must not, as sometimes happens, suffer for the sins of the wireworm, of the larva of the cockchaffer, and of other depredators, whether insects or cryptogamous plants.

Earthworms may sometimes cause annoyance by boring their way in or out of the soil among young seedlings, and displacing them. But neither this sin nor that of slightly disfiguring lawns with their casts can be brought into comparison with the great good which they unquestionably occasion to the farmer and the gardener.

The only really serious charge against worms is that of bringing infectious matter to the surface of the soil. But this danger we may easily avoid by treating animal matter before burial with some of the many chemicals which worms avoid.

It has been even proposed to introduce worms in localities where they are deficient, in order to ameliorate the soil. For this purpose a hole is dug out, perhaps a yard in depth and as much in diameter. If the bottom of the soil is of a very compact and retentive nature, it is somewhat broken up with a pickaxe or a crowbar. The hole is then filled up

with moist sand, and receives a top-dressing of leaf-mould and rotted dung, and a few healthy worms are introduced. The operation should of course be undertaken in moist, calm, cloudy weather.

VI. ON TECHNICAL EDUCATION.

By Robert Galloway, M.R.I.A.

(Continued from page 29.)

OUR inquiry into the Education system adopted by the Department of Science and Art will be confined chiefly to the Science division; but it would be as desirable to have what may be termed an accurate "stock-taking" of the *practical results* achieved by the Art, as well as the Science, Schools: it would be very desirable, for instance, for the nation to know the proportion of French to English designs produced at the present time for our calico-printers, and the amounts paid respectively for each. Mr. Potter, the then largest calico-printer in the world, stated, in the evidence he gave before the Select Committee of the House of Commons in 1864, that prior to 1840 there was a considerable remuneration to the designers at home: we most of us, he said, kept up large establishments, and to my certain knowledge there were many men receiving from £100 to £500 a year as designers for the trade; I believe, he said, no such sums are now (1864) paid in this country. We are more than ever reliant upon the Paris *ateliers* for designs. I have, he said, made a calculation which I believe to be within the mark; I know what certain houses pay—£2000 or £3000 a year—in Paris for designs. We have regular agencies there, which we had not even a few years ago. I believe the amount paid by calico-printers *alone*, at this very time, is enormous. I may state, at once, that I know twelve houses that pay from £25,000 to £30,000 a year for designs. I believe the entire payment now in the trade for French designs alone is upwards of £50,000 a year: we get much better designs in Paris; unless it were so we

should not go there, of course: we have, in the meantime, *lessened our own designing power very greatly.*

Fifty thousand a year must have been by no means an unimportant factor at that time in fostering art in Paris, by giving employment to no inconsiderable number of French artists: in any inquiry, therefore, at the present time, into the technical education of this country, it ought to be a matter of great importance to ascertain accurately whether our calico-printers are still indebted to French artists for the majority of their best designs.

Some at least of our Statesmen and Members of Parliament, and others of the community, have been led to believe that the system adopted by the Department for teaching Science in Elementary Science Schools is both efficient and economical, and that no other system could produce equal —not to say better—results at the same cost to the State. I think it can be shown—and shall now attempt to show, and I hope convincingly to the majority of impartial persons—that a system more in accordance with sound educational principles, and which would therefore produce better practical results for the nation, could be devised, and would, at least, be no more costly to the State than the present one.

In the evidence I shall adduce to show the defects and deficiencies of the present system, I shall take my facts from three sources:—1st, Public documents. 2nd, From personal knowledge. 3rd, Facts that have been accidentally given to me by gentlemen who have taught under the Department. I say accidentally supplied to me, because I did not go in search of them; and I shall be most careful in stating them —they are not in the smallest degree distorted. And this question of whether we have or have not adopted the best system for the teaching of subjects the practical applications of which ought to lead to the improvement of old, and the establishment of new, industries in the country, is by no means an unimportant one for us at any time; but it is especially important at present, when we as a nation have to face a stagnant, if not a declining, revenue, occasioned by a serious falling off of our exports both in *quantity* and *value*, owing to increased and increasing foreign competition.

It has already been pointed out that under certain circumstances the teacher may receive no payment from the Department for his year's work; but there are other exceptional cases, besides those stated in the Science Directory, which will forfeit the teacher's earnings. I will relate two that have been brought under my notice:—A few years ago

a gentleman started, and taught, a chemical class in the Mechanics' Institution in this place. The examination was duly held; but when the teacher sought for payment he was informed that owing to some informality the Secretary of the Local Committee had committed—of which he, the teacher, was up to that time totally ignorant—no payment could be given to him, nor prizes awarded to the students.

When Physical Geography was one of the subjects in the Departments course, a gentleman, who afterwards related the particulars to me, started a class in the subject, and gave about a hundred lessons. The class knew their subject thoroughly; and a few days before the official examination he, the teacher, held an examination, and out of twenty questions he gave eight of them comprised the examination paper of the Department's examiner, but the pupils were awarded no marks at the Department's examination; the teacher therefore received no payment, and he consequently gave up teaching the subject. Owing to local peculiarities the pupils' vocabulary of English words was somewhat limited, and therefore they all expressed themselves very much alike; and as they were acquainted with the answers to the eight questions the Department's examiner gave, the teacher concluded that the Departmental authorities believed that the pupils had been made acquainted with their examiner's paper before going into the examination room. This was afterwards apparently confirmed in the following manner:—Some time afterwards the same gentleman taught, in the same place, a class in Geology. "The answers"—I give the gentleman's own words—"were so good that the Department would not credit my class for having written them, and an Inspector was sent who boldly accused us of unfair dealing. The candidates were asked to write the answers again, which they did, though it was some six or seven weeks after the examination. I told the Inspector I would find no difficuly in 'spotting' their questions; I showed him a few papers of questions in my note-book which would ensure answers to their questions: they allowed some on the second occasion to pass. The Local Committee complained of the unfairness of suspecting the work done by the pupils simply because their expressions were alike, if not identical." This resulted in this gentleman abandoning the teaching of that subject also.

When payment has been awarded there is frequently great delay in paying the grant. On January 23rd, 1882, there appeared in the "Standard" newspaper a communication from "A School Manager" respecting the delay in the

payment of the Government grants to schools; on the 26th of that month there appeared in the same newspaper the following letter:—

"*The Payment of Science Grants.*

SIR,—May I be permitted to supplement the letter of "A School Manager" to point out that the delay in the payment of Science Grants is even worse than in Elementary Schools. Science Classes commenced in September, 1880, were examined in May, 1881, the results published in July, and the payments made in December. Most Science teachers are paid solely out of the grant. Is it not sufficient that teachers alone, of all the professions, should voluntarily undertake a whole session's work without touching payment, but they must have a gratuitous addition of five months' waiting after the results are declared? One Science teacher, who has since died, whose enfeebled constitution needed the support, and whose nervous temperament chafed against the delay of his grant (some £40) received it on the Friday before Christmas-day.

"I am, Sir, your obedient servant,

"SCIENCE TEACHER."

Even if the uncertainty of receiving any payment at all, and the uncertainty when it will be paid even when it has been awarded, affected only the Teacher and not the Taught, it must be admitted, I think by every one, that a system which involves all this uncertainty about the teacher's pecuniary remuneration requires at least some reforming; for it can scarcely be doubted that this uncertainty must act unfavourably in securing the best talent, and that it will only be adopted as a last resource for making a living. But these uncertainties must more or less, according to the temperament of the teacher, unfit him for discharging as efficiently as he would otherwise do, if his mind was at ease with regard to pecuniary matters, the duties of his calling. But we will now pass on to the defects and deficiencies of the system as they more immediately affect the taught, and therefore through them the nation.

Written examinations are mainly the Department's tests of the candidate's knowledge of the Inductive Sciences: the teacher, to have any chance at all in securing any payment from the Department, must, on that very account, teach his pupils to pass; whether they know the subject they are taught must necessarily be to the teacher, as far as his

self-interest is concerned, of very little or no importance compared to that of their passing: he will therefore devote special attention to those who are most likely to pass,—to those who have, he finds, the best verbal memories; and that this really takes place will be proved, I think conclusively, when the number of pupils in the different Science Schools are contrasted, which will be done in a future article, with the number that present themselves for examination in the elementary stage, and these again with the number which go in for the highest, honour, examination. Here is a fundamental difference between the system of teaching carried on under the Science and Art Department and the system carried on in Germany. "It is our principle," stated an eminent German teacher in one of the Trade or Business Schools in that country, "to adapt our instruction to the wants of the *average* boy,—to see that he is brought up to the prescribed mark at the proper time;" and if England wants a good educational system—one that will be for the advantage of the nation, whether it be *general*, *scientific*, or *technical*—it must also be one principle; but this cannot be as long as we adopt the system of payment on results, especially as that system is carried out by the Department of Science and Art.

Another evil of the payment on the result system is that it compels the teachers, owing to small sums they can earn by teaching one or two subjects to one class, either to teach many subjects in one or two schools, or to teach one or more subjects in several schools; the teachers have, therefore, to give so much time to teaching, in order to obtain little more than a bare competency, that little or no time is left them for prosecuting their own studies so as to keep them on a level with the progress which is taking place in the subjects they teach. In what a different position, in this respect, is the teacher under this system to the teacher in Germany; in that land of learning *three lessons a day* is considered very full work for a teacher. It is thought in that country that a teacher should give no more lessons than he can give with the whole force and freshness of his mind without undue exhaustion; and, above all, he should have time for prosecuting the private studies which enhance the value and efficiency of his work, and he is only called upon to give instruction in *cognate* subjects.

In looking over, some little time ago, the Department of Science and Arts Report, published in 1881, I noticed many facts supporting what I have just stated as regards the time the teachers paid on the result system have to give to

teaching: one teacher taught, according to the Report, in no less than *nine* distinct schools; another taught in *eight* schools under the Department, and in *one* at least under the City and Guilds of London Institute; and several, as shown by the Report, in *six*, *five*, and *four* different schools.

A gentleman told me a few years ago that in order to gain a living he had, in addition to teaching in a day school, to teach evening classes, and some of the places where these were held were so far removed from where he lived that he did not reach home sometimes until 2 o'clock in the morning; he added, it was killing work. Another gentleman has stated to me—"I certainly am not contented with the work I have to do; 10 a.m. to 9 p.m. three days a week, 10 to 5 two days, 10 to 1 Saturdays; these are my hours, and I teach no less than *eight* Science subjects. From all this, if the *results are good*, I make about £250 a year; chance of promotion none. I really feel that I am throwing the best years of my life away in useless drudgery, for I do not like (*or know much about*) the large majority of the Sciences I teach.

I think our Commissions need not go far from home to learn why we are so far behind other countries in scientific and technical education. A true instructor adapts his methods and processes of instruction to the mental capacity of each of his students; but how can a teacher who has either many subjects or many classes to teach do this? it is both a physical and mental impossibility. Prof. Fleeming Jenkin considers that, even under the most favourable circumstances, it is very difficult to obtain Science teachers capable of rendering Science as a good and mental discipline, as the teachers of the dead languages made their subjects: "for this reason Latin and Greek were dead languages, and did not change; they were taught now as they were fifty years ago; but Science changed with inconceivable rapidity, almost from day to day. What were the first-fruits of Science last year was already antiquated, and *the difficulty was* how to get a body of teachers who could follow all these constant changes at the same time they were imparting instruction in secondary schools. He did not say it was impossible, but there was a great difficulty."

(To be continued.)

ANALYSES OF BOOKS.

Colin Clout's Calendar. The Record of a Summer—April to October. By GRANT ALLEN. London: Chatto and Windus.

WE have here another of those bright, chatty volumes in which Mr. Allen is rendering the New Natural History into the language of daily life, and incorporating it with the stock notions of the public. This is a much-needed task, and the author takes it in hand not merely with a thorough good-will, but with a rare felicity.

The first interesting observation we find refers to the difference of ground selected by each of those closely kindred flowers the primrose and the cowslip. Both of course love a chalky or calcareous soil, and are scant and poor where lime is deficient. But the primrose, Mr. Allen tells us, grows chiefly on sloping banks and the sides of little hillocks, whilst the cowslip is found in abundance in the midst of level plains. This distinction is noted in an old song which we heard some thirty years ago,—the fossil fragment, so to speak, of an opera which has been duly consigned to extinction for the twofold sin of English music and English words:—

> "Primroses deck the bank's green side,
> Cowslips adorn the valley."

This simple fact will give us a good illustration of the difference between the two Schools of Natural History. The Old School would have simply noted it as a fact. The New, or Evolutionist School, brings the fact into connection with certain features in the growth of the two plants. The cowslip carries its flowers in a cluster at the summit of a stout stalk, high enough to overtop the grass at that early season of the year when it usually comes into bloom. The primrose has its flowers singly, each supported on a stalk too short and slender to bear them up above the neighbouring herbage. Hence were they to grow in the same localities the primrose would be likely to escape observation, whilst the cowslip would be seen very well. Thus the primrose haunts sloping grounds where the shortness of its stalk does not occasion concealment. Hence the difference of ground is not an arbitrary matter, but is in connection with the structural features of the two plants.

But a botanist of the first quarter of the century might and would still ask, What would the primrose suffer if it escaped observation? If this were the case it would not be visited by bees and butterflies, and would consequently lose all the benefits of

cross-fertilisation. Thus we see how in the light of Evolutionist principles such a stray fact as the favourite locality of a flower acquires a meaning, and how it in turn aids in confirming those very principles.

On the colours of flowers the author makes, in passing, some interesting remarks, from which a lesson unwelcome to the modern æstheticists may be drawn. These good people, for some reason not very apparent, have an especial love for yellow flowers. Now as any truly philosophical botanist well knows, and as Mr. Allen does not fail to point out, "the most primitive flowers are yellow; those a little above them in the scale have usually become white; those more highly evolved are generally red or pink; and the highest grade of all, the blossoms peculiarly modified for bees and butterflies, are almost always blue or purple." If we turn again to consider leaves we find a yellow colour associated with decay. And if we consider the processes by which artificial colours are manufactured, we shall find that inattention, carelessness, too high a heat, or an excessive prolongation of the action, will give other colours a yellow cast or turn them altogether yellow. Hence the worship of the sunflower and the daffodil is altogether an aberration, from which a fine feeling for the beautiful would preserve even the unscientific mind. A true æstheticism will transfer its love and reverence to red, purple, and blue flowers. We may here see, also, the reason why it is rare to find yellow and blue species in one and the same genus, or yellow and blue varieties in the same genus. We see also the reason why yellow flowers are so abundant; they are within the scope of Nature's "'prentice hand."

"The Trout Jump" is a curious medley of interesting matter, leading sometimes to difficult philosophical questions. The author writes:—"One of the great dangers, indeed, which surround the path of scientific psychology is that of being too exclusively human. Here, more than anywhere else in Science, the old Greek doctrine that man is the measure of all things, seems especially to beset us on every side." This saying may be, and is, taken in two senses, so different that the one may be pronounced nonsense and the other a profound truth. "Man the measure of all things" is commonly understood to mean that everything in the world exists with reference to man, and is arranged with a view to his interest and convenience. In the other or idealist meaning it is simply an admission of the purely phenomenal character of all our knowledge.

In the same chapter the author introduces his hypothesis that "in every case a taste for colour has been aroused in the creatures themselves during their constant intercourse with brilliant surroundings and their continual quest for brilliant articles of food."

The rule, however, that the most gaily-coloured species are flower-feeders or fruit-eaters, is not without signal exceptions, as Mr. A. R. Wallace has indeed pointed out. It fails in such groups

as *Cicindela*, *Calosoma*, in the more splendid Carabs, in many of the Buprestidæ, the Longicornes, the Phanæi, among birds in the pheasants, the trogons, and in certain lizards and serpents.

In a chapter on "Catkins and Almond Blossom"—which latter he does not think pretty—Mr. Allen states—"None of our own English trees has such a brilliant spring blossom as the flowering almond." He must forget the Mezereon, whose flowers completely anticipate its leaves.

We note the remark that "you will never find a brilliant mass of heavy bloom on a strictly annual plant, and all the more massive forms are provided for beforehand by means of bulbs, corms, or tubers." This generalisation is followed up by an ungracious sneer at certain plants which are mostly admired. He speaks of "deceptive and papery flowers, like the poppies or the hand-to-mouth convolvulus, which manage to make a great deal of show at very little real expense. They spend all they have on a little gaudy colour, thinly spread out over an extremely large flat surface." The man who can speak thus of the morning glory cannot be implicitly trusted on questions of beauty.

Speaking of the "Mole at Home," Mr. Allen tells us that "wunt or wont is the good old English name of those underground animals which we now-a-days know as moles." We doubt if the name "wunt" was ever known in the north and east of England. In the most retired and old-fashioned parts of Lancashire and Yorkshire they were known as "mowdiwarps" or "mowdiwarts," *i.e.*, the animals which warp or turn the mould. The author does well to remind his readers that the common diagrams of the mole's underground habitation are "ridiculously overdone in the matter of systematic completeness."

Double flowers do not meet with Mr. Allen's approbation, and the following outburst is really unpardonable:—"Our cottage garden will not away with your modern Gloxinias, and heaven only knows what other newfangled things called by doubtfully classical names unlovelier than themselves." If the Gloxinia is unlovely, the same must be said of every other flower, and of most with a fearful *a fortiori*. And yet Mr. Allen can speak of the "ornamental value of the common wind-fertilised plants," which he himself admits bear upon them the mark of degradation and degeneracy. He says "our ordinary taste in the matter of flowers is still a trifle barbaric." What if we say that the decreasing fondness for bright colour is a mark of racial degeneracy, just like the love for bitter beverages, which seems to spring up in a nation just as dyspepsia and general debility become more common? Pure bright colours may be ill-assorted, but that is no justification for the sage greens and other dirty shades now coming into request.

We regret that we cannot carry our examination of this work

any further. Though containing much that we cannot accept, it contains more that must be pronounced of exceeding interest, and no small stock of original and accurate observations.

Physical Optics. By R. T. GLAZEBROOK, M.A., F.R.S. London: Longmans and Co.

THIS work forms one of the series of "Text-Books of Science" which are being brought out by the Messrs. Longman. The author in his Preface explains the division of the science of light into Geometrical and Physical Optics, the latter of which deduces the laws of the propagation, reflexion, and refraction of light from a certain hypothesis, and explains at the same time certain phenomena which geometrical optics fail to account for.

Even in this Preface the author refers to the difficulty of explaining the existence of rectilinear rays of light on the principles of the received undulatory theory. He seems almost to apologise for the simplicity of the mathematical methods made use of, which he fears "will appear long and clumsy to those who are acquainted with the differential and integral calculus." At the same time many readers will think that he has made quite sufficient demands upon their attainments.

In the first chapter we find an account of the conservation of energy, of light as a form of energy, of the Newtonian emission theory, and of the difficulties which led to its ultimate rejection. In the next chapter we have an explanation of the undulatory hypothesis, preceded by a notice of the assumed ether as the medium by which the light-vibrations are transmitted. Reasons for believing the existence of the ether are promised in a subsequent portion of the work. In the same chapter the author explains the rectilinear propagation of light,—a difficulty which Huygens, the originator of the undulatory theory, failed to overcome, and which was Newton's chief objection.

Chapter III. deals with reflexion and refraction, and explains the connexion between velocity, wave-length, and period.

In the following chapters the author treats successively of prisms and lenses; of interference; the colours of thin plates; diffraction, dispersion, and achromatism; spectrum analysis; absorption and anomalous dispersion; double refraction; the reflexion, refraction, and interference of polarised light; circular polarisation; electro-optics, and the velocity of light.

Under the last head the author discusses the hypothesis of Messrs. Young and Forbes, that the blue rays travel more rapidly than the red, and concludes that in air, as in a vacuum, waves of all colours travel at the same rate.

In treating on electro-optics he concludes that the ether and the electro-magnetic medium are the same, and that, as Maxwell puts it, "light is a series of oppositely directed magnetisations and electro-motive forces."

Mr. Glazebrook's work seems exceedingly well suited for the class of readers to whom it is more especially addressed.

The Relative Mortality after Amputations of Large and Small Hospitals, and the Influence of the Antiseptic (or Listerian) System upon such Mortality. By H. C. BURDETT. Reprinted from the "Journal of the Statistical Society." London: J. and A. Churchill.

IT strikes us that the long title of this little work might be improved by the substitution of "in" for "of" before the words "Large and Small Hospitals." As the title now stands a captious person might ask whether the hospitals themselves had been amputated instead of the limbs of the patients?

Apart from this trifling slip in language we find here much that deserves attentive consideration outside the pale of the medical profession. The results of the figures given show distinctly that the probability of recovery after a serious operation is greater, tangibly greater, in the so-called "cottage hospitals" than in the larger establishments. Hence Sir James Simpson cannot have been far wrong in maintaining that there exists "an inherent unhealthiness in large hospitals." The attempt to account for the difference, as one of the author's critics has done, by the assumption of a "difference in the surgical practice," is a curious plea, since it would amount to something very like an admission that the surgical skill at command of the great city hospitals was inferior to that brought to bear in the cottage hospitals. Still more curious is the assertion, by the same critic, that the "arrangements for nursing, cleanliness, and ventilation in cottage hospitals" are far inferior to those of the city hospitals. This statement is either true or false. Mr. Burdett proclaims it "entirely imaginary and contrary to the fact." If true, so much the worse for the critic in question; for there must then, on his own showing, exist in the city hospitals some cause of mischief potent enough to overpower the advantages of the superior cleanliness, ventilation, and nursing which he claims for the large establishments.

We are glad to find that the author fully appreciates the dangers of the water-closet and water-carriage system. He shows that in the original cottage hospitals earth-closets or old-fashioned privies outside the buildings were used. Now too many of these

hospitals show a desire to "dabble in bricks and mortar." The new buildings are fitted with water-closets placed in the interior of the buildings, and thus sewer-gas is laid on, and conveyed almost to the bed-side of the patients, whilst drains are made, as in the majority of modern dwelling-houses, to run underneath the building. It is lamentable how completely men of science, commonly so-called, forget that a water-joint does not prevent the interchange of gases.

The results of the Listerian system in surgery are proved to be decidedly successful. The author, in pointing to the value of the work of such men as Lister, Wells, and others, asks pointedly why the highest honour ever earned by a physican or a surgeon is a baronetcy, whilst lawyers and financiers may aspire to and frequently attain peerages? Walter Savage Landor complained that the "blackest mud of the Bar and the Exchange" was "shovelled up against us in heaps."

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*Der Chemismus, Magnetismus, und Diamagnetismus in Lichte mehr dimensionaler Raum-anschauung.** By RICHARD BROSCH. Leipzig: R. Brosch.

OUR readears will doubless admit that we have here to grapple with a perplexing and a thankless subject. To ignore it because it seems not easily reconcilable with the orthodoxies of modern science is contrary to our custom, and to our notions of duty. To form, as far as we are able, a fair and impartial estimate of its value, is to offend two opposite parties, each of which tacitly demands from us a benevolent neutrality. The subject has also its inherent difficulties. To judge of an attempt to consider chemism (*i.e.*, the totality of chemical phenomena), magnetism, and diamagnetism, in relation to a concept of space of more than three dimensions, a clear understanding of such space, and of what it involves, is essential. But such a clear understanding we do not possess. We do not deny that possibly to beings higher than man—the existence of which in the universe we here neither affirm nor deny—space may perhaps appear to admit of four dimensions. But this consideration does not assist us in the least. Perhaps our best course will be to quote from the work before us a popular presentation of the matter, which Herr Brosch, curiously enough, borrows from an opponent,—Professor Vogel.

"We measure space according to a threefold extension,—

* Chemism, Magnetism, and Diamagnetism in the Light of a Poly-dimensional View of Space.

length, breadth, and depth,—and we have a clear conception of a simple body, *e.g.*, a quadrangular pillar, when we state its dimensions in length, breadth, and height. Concerning a fourth dimension we know nothing, and are absolutely unable even to conceive it. But this may possibly be due to the limitation of our senses. Let us imagine a being which, in virtue of its peculiar organisation, lived in a plane, and was incapable of recognising any object outside of such plane. This being will possess a conception of the extent of its plane in length and breadth, but, to it, the conception of depth or height—and consequently of space—would be totally wanting. All objects would become visible to this being only when they entered its plane of perception, and on leaving it they would vanish. A human finger touching this plane with its point would, *e.g.*, become visible to such a being as an oval spot at the point of contact, whilst the part of the finger standing above the plane would remain, to it, invisible. If the finger touches the plane, not with its tip, but with its side, our supposed being will obtain a quite different view of the finger, which will now appear as a long bent figure. Such a being will scarcely believe that the oval spot and the long figure proceed from one and the same object. If we remove the finger it will vanish from the sight of the supposed being, who will be unable to conceive what has become of it. Let us suppose a circle drawn in the plane; it will appear to our imagined bi-dimensional being as a perfectly closed figure. That the circle is open above and below it will not perceive. If it wishes to go to the centre of the circle it must break through the boundary-line, and if we, from above or from below,—*i.e.*, *from the third dimension*,—put a finger in the middle of the circle without breaking through the circumference, it will, to the being in question, appear a miracle.

"It would be quite the same with us if there existed, in addition to the three dimensions of space which we perceive, length, breadth, and thickness, yet a fourth, which we do not perceive. If an object passes into the fourth dimension, it vanishes for us, and suddenly re-appears when it enters into our region of perception. It is even conceivable that a hollow ball, which to us appears as a body closed on all sides, is open in the fourth dimension, just as the circle which to a bi-dimensional being appears completely closed, is open above and below.

"To a bi-dimensional being the tying an ordinary knot on a piece of string free at both ends, would appear inconceivable, as for this purpose one end of the string must be lifted up out of the plane in order to be passed over and then under the other part of the string, whilst a man, with his conceptions of height, breadth, and length, at once understands this operation.

"On the other hand, we are not able to tie a knot in a string whose two ends are connected so as to form a ring without first opening the string. But if we suppose a fourth dimension, and

beings which can act in it, it is conceivable that such beings may be able to tie a knot on a ring-shaped string."

This is, perhaps, as clear a representation of the hypothesis of poly-dimensional space as can be obtained without the aid of transcendental geometry.

The author propounds a hypothesis which he endeavours to follow out to its physical and chemical consequences. It is as follows :—"Our tri-dimensional world, in as far as it is composed of ponderable matter, consists of poly-dimensional atoms, arranged in a tri-dimensional manner."

This hypothesis compels us to the conclusion that every atom possesses several magnetic axes, situate in various fixed directions in or upon it. Those only which lie with both their poles in tri-dimensional space are permanent and incapable of being induced. All the others are ordinarily inactive, but owe their origin to an inductive action. According to the position of the atom sometimes one and sometimes another of these axes comes to lie in tri-dimensional space, and furnish then permanent atom-magnets.

This hypothesis, further, according to the author, serves to explain how one and the same atom, in different combinations, can appear with a different atomicity.

But, admitting that the hypothesis, if fully followed out, may throw a new light upon a multitude of phenomena, we have still to ask what are its foundations? For, if no fourth dimension exists, the light thrown by the new hypothesis will be a Will-o'-the-Wisp,—an *Irrlicht*. We must ask, then, for a perfectly distinct and decisive proof of the existence of this fourth dimension. There have been produced, we are told, certain phenomena, such as the tying of a knot on a closed string, the passing of two wooden rings through each other chain-fashion, the passing of a ring upon a pole at either end of which there are projections whose diameter exceeds that of the ring, the placing of a ring upon a man's arm whilst he holds his hands clasped together, &c. Admitting all this without discussion,—for which, indeed, our space is wholly insufficient—there remains a difficulty which Herr Brosch has not ignored. Whether he has succeeded in meeting it is another question.

Supposing the above-mentioned phenomena verified, are we to infer, with the late Professor Zöllner, and with our author, the existence of a fourth dimension, and of beings capable of acting therein? Or are we to assume, with not a few believers in the reality of the phenomena, that there exist superhuman beings capable of abolishing temporarily the continuity of an iron ring, of a wooden pole, or of a human arm, and then of restoring it to its former condition? We do not find here any answer which compels our belief.

That the work before us is suggestive and that it is worthy of a careful study we gladly admit. But we seek to know whether

the fourth dimension is a *vera causa* before tracing out its possible consequences.

The Land Junction of Great Britain and Ireland by an Isthmus at the Mull of Cantyre. By J. CHARLES KING, Engineer, &c. London: R. H. Squire.

WE have here in a brief pamphlet a proposal of startling magnitude. The author proposes nothing less than an artificial isthmus running from Torgor Point on the north-east coast of Ireland to the south-western extremity of the Mull of Cantyre, thus uniting Britain and Ireland, and converting the Irish Sea into a land-locked bay. The project cannot be pronounced other than bold. But it is certainly practicable if the Legislature grants permission, and if the needful capital—which will not be trifling—can be obtained. The water way to be built up is, according to the author, 19 miles in width, about 200 feet in depth near either coast, but 474 feet deep in the middle. The current running from the north-west into the Irish Sea is given on the author's illustrative chart as six knots per hour in the middle and at the Irish end of the proposed embankment, but as "of great strength" near the Scottish coast. Still these difficulties are doubtless not beyond the resources of modern engineering. It is mentioned that there are at both ends eminences from which the materials could be obtained in sufficient quantity, and could be run down to the sea by gravitation alone. Near the Irish coast there are, indeed, certain hills ranging from 980 to 1300 feet in height, which would yield almost a sufficiency of rock, &c., as the proposed isthmus is to be only about 100 yards in width. The author's estimate of the expense of the undertaking is about two millions sterling. We must not forget, however, that the actual cost of such great undertakings generally falls out very much higher than the original estimate. Perhaps if we say £5,000,000 we shall be on the safe side. The time required for the completion of the work is stated as from two to three years.

As regards the cost the author makes some very important suggestions. He does not propose an outlay of national funds, and a consequent increase of taxation. For labour he proposes to make use of the able-bodied convicts, now uselessly or harmfully employed in prisons. He estimates that some thirty thousand such men could be put to the work at once, and he thinks that "the gain to the state by closing their prisons in and near London alone would yield valuable ground worth many thousand pounds for yearly rental."

But, on the other hand, the thirty thousand criminals would

require to be housed, fed, and adequately guarded. For these purposes at least two temporary prisons of great size would be needed, one in Ireland and one in Scotland. The scene of operations on each coast would need to be invested by a cordon of troops, and to prevent escape by sea a proper and efficient blockade would have to be maintained along both the northern and the southern coast of the isthmus, especially as it approached completion. Nor do we see that the ordinary prisons, in London or elsewhere, could be permanently closed, for, without considering the many convicts who would be manifestly unfit for such labour, what would have to be done with the able-bodied ruffians when the work was completed?

Mr. King's method of raising the necessary funds seems to us very judicious. He proposes that "the tolls and land-rentals of so important a position as the isthmus would become for a new city, might be hypothecated by a paper currency issued under State guarantees for this special object." This plan was, if we remember rightly, adopted for the erection of the Jersey meat-market and proved most successful.

We must now glance at some of the more formidable difficulties in the way. The first consideration is the interference with navigation. A glance at the map shows us that the sea route from all ports on the west coasts of England, the south-west of Scotland up to and including the estuary of the Clyde, and the east coast of Ireland, would be greatly interfered with. The coasting trade between all those parts and the west of Scotland or the north of Ireland would be obliged to seek another channel. Without having any statistics at hand we should conclude that the greater part of the commerce of Glasgow with Canada, the West Indies, the United States, &c., passes along the North Channel between Ireland and the Mull of Cantyre. A considerable share of the trade of Liverpool and of Belfast will take the same direction. To meet this difficulty Mr. King proposes to widen the Canal of Crinan, and to cut another through the Isthmus of Tarbert. These projects would involve a certain outlay, and hence a toll would be necessitated. A ship-owner in any of the ports concerned would say with justice: "At present I have a comparatively short and simple route provided by nature. You block it up and compel me to take a longer and more intricate track, and for so doing you charge me a toll!" Hence we conclude that on mere commercial grounds the project will meet with warm opposition from the sea-ports in question.

There is another difficulty to be encountered. The current flowing into the Irish Sea through the North Channel is, as everyone knows, and as the author expressly mentions, a part of the Gulf Stream. To its action the south-west of Scotland, the Isles of Bute and Arran, the shores of the Frith of Clyde, certain parts of the north-east of Ireland, and even the coasts of Cheshire, Lancashire, and Cumberland, owe their exceptionally

mild climate. The vegetation of Bute and Arran reminds us of the Channel Islands, the Scilly Isles, and the most sheltered spots of the coast of south Devon. Everyone must know that the districts of Chester, Warrington, Lancaster, Ulverstone, &c., are often enjoying open weather when the midland counties, even Kent and Sussex, are covered with snow. But supposing this influx of the Gulf Stream cut off by the proposed isthmus, there is every probability that the temperature of these districts would be reduced to that which prevails at corresponding latitudes on the eastern side of Britain. Bute, Arran, Helensburgh, &c., would lose much of their attraction for health- and pleasure-seekers.

It is also very possible that the fisheries on the north-western coasts of England and on the south-west of Scotland would suffer, *e.g.*, the Lock Fyne herring fishery.

On the other hand, the land junction would, in the opinion of the author, lessen the depth of the Irish Sea, "serving to aid the natural drainage of the adjacent coasts, and make the marsh-land of the west (?) coast of Ireland capable of the highest forms of cultivation."

Another advantage which suggests itself is that at present in case of war an enemy's fleet might enter the Irish Sea and do incalculable damage to the wealthy and practically undefended towns of our western coasts with but little fear of being intercepted. Were the proposed isthmus an existence, such a fleet would almost inevitably find its retreat to the south cut off.

What may be the general opinion of the nation should this project take a more definite shape, we can scarcely venture to foretell. But we suspect that in Ireland and in certain parts of England and Scotland, it will meet with but little support.

Journal of the Royal Microscopical Society. Containing its Transactions and Proceedings, and a Summary of Current Researches relating to Zoology and Botany (principally Invertebrata and Cryptogamia), Microscopy, &c. Edited by FRANK CRISP, LL.B., B.A., one of the Secretaries of the Society, with the assistance of the Publication Committee, and A. W. Bennett, M.A., B.Sc., F. Jeffrey Bell, M.A., S. O. Ridley, M.A., and John Mayall, Junr. Series 2, Vol. II., 1882, pp. 941. London and Edinburgh: Williams and Norgate.

THE current volume of this valuable periodical has just been issued. It contains first of all sixteen original communications

read at the meetings. The value of these is greatly increased by the publication in another part of the book of the discussions following the reading of the said papers, a course which has always been the practice of the Royal Microscopical Society.

As may be expected from the extensive use of the microscope as a means of reseach, the papers are of a varied description. Those on biological subjects comprise: "Further notes on the British Oribatidæ," by A. D. Michael, F.L.S. The strange forms of these minute creatures, so accurately figured by the author in this and many preceding communications, must attract the attention of the most casual reader. Mr. Michael is preparing the results of his observations for publication on a more extended scale than is afforded by the limits of an occasional paper. Dr. John Anthony's communication "On the Threads of Spiders' Webs," places on record some interesting facts respecting the structure of the web of *Epeira diadema*. Dr. R. L. Maddox contributes two papers, "On some Micro-Organisms from Rain-water Ice and Hail," and "On some Organisms found in the Excrement of the Domestic Goat and the Goose." "Note on the Spicules found in the Ambulacral Tubes of the Regular Echinoidea," by Professor T. Jeffrey Bell, M.A., one of the editors of the Journal; and "On the Bacteria of Davaines Septicæmia," by G. F. Dowdeswell, M.A.; note by F. Kitton on Rev. G. L. Mills's paper on "Diatoms in Peruvian Guano"; "Plant Crystals," by Dr. Aser Poli, complete the biological portion.

Among apparatus described are a "New Growing or Circulation Slide," by T. Charters White, and "A further Improvement in the Groves-Williams Ether Freezing Microtome," by J. W. Groves. Mr. Stephenson's* communication on "Mounting Objects in Phosphorus and other Dense Media," and Dr. B. Wills Richardson's, "On a Simple Process of Imbedding Tissues for Microtome Cutting," complete the papers on manipulation.

Two portions of a most valuable optical paper by Professor Abbe, of Jena, entitled "The Relation of Aperture and Power in the Microscope," appear in the present volume.

The address of the President, Professor P. Martin Duncan, is chiefly occupied by an admirable exposition of recent optical discoveries. Calling attention to the small number of papers received by the Society, he reminds his readers that a very large number of observations principally worked out by means of the microscope have been recorded during the past year in the transactions of other societies, and that the microscope is always being carried into new fields of research. It promises to be of great assistance to the chemist, and while but a few years ago no one thought of including it among the essential tools of the geologist, it is extensively applied to the examination of rocks, and most valuable results have been brought to light by its aid.

* Journal of Science, vol. iv., p. 376, 1882.

He congratulates the Society that whether as regards theory or practice—the optical and mechanical or the observational part of our science—there has never been a time when so much evidence could be produced of solid progress as now.

The "Summary of Current Researches," which occupies the greater part of the volume is a most valuable compilation, and the patience and energy of the editors in collecting and preparing so complete an abstract of biological and microscopical research cannot be too highly commended. Preference is as a rule given to foreign papers, for the reason that such are not very accessible to English readers, while memoirs published at home need little beyond a reference to the place where they may be found, but even here very useful abstracts are given.

The index and table of contents are both very copious and admirably arranged, so that a subject sought for may be traced out with the minimum of trouble. In addition a very complete list of books and papers, British and foreign, relating to biological and microscopical subjects is given.

With each separate number appear a valuable series of tables compiled by Mr. J. W. Stephenson, F.R.A.S. They will be found very useful to microscopists. They include the respective values of "numerical and angular aperture" in air, water, and oil; the conversion of British and metric measures; corresponding degrees in the Fahrenheit and Centigrade scales; and, lastly, a table of magnifying powers.

Longman's Magazine, No. III., January, 1883.

None of the articles in the issue before us deal with subjects upon which we may claim a right to adjucate. To a few incidental passages we may, however, refer. Thus, in the serial story, "Thicker than Water," we read: "The dog is to be won by fear, but the cat never—a circumstance which, if there were no other urgent reason for it, would always place the cat above the dog, in my opinion." "The New Pygmalion" is a clever allusion to a trial which has enriched the lawyers, and has, to the general public, proved only less wearisome than that of the "Claimant."

That strange ghost story, "The Lady's Walk," comes to a conclusion. Evil is averted, but not by the mysterious spiritual guardian, whose part thus becomes little better than that of a supernumerary.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

THE NICETY OF BIRDS.

To the Editor of the Journal of Science.

SIR,—The following facts will perhaps not be unwelcome to that numerous portion of your readers who take an interest in animal psychology:—

A canary-bird belonging to an acquaintance is exceedingly fond of bathing. As he makes a considerable slop in so doing, the lady to whom he belongs placed his cage in the kitchen till he should have taken his bath; but the bird on such occasions invariably refused to enter the water till he was brought back into the breakfast parlour.

Another canary had been accustomed to have his bath in a cup of black glass. It having been broken, a common earthenware cup was substituted; but the bird refused to bathe till a glass vessel (this time colourless) was obtained.

A large grey parrot belonging to a neighbour of mine is much offended if his cage is not cleaned out regularly. On one occasion, having been neglected for nearly a week, he fell to work, and with beak and feet contrived to throw all the sand, fragments of food, &c., which had accumulated at the bottom of the cage, down upon the carpet.

These facts show that birds have a sense of order, neatness, and even elegance, and that their whole nature is not absorbed in the gratification of their physical wants.—I am, &c.,

H. M.

"FARINI'S MISSING LINK."

To the Editor of the Journal of Science.

SIR,—Having paid a visit to this marvel I must confess that I see little evidence for its being a form connecting man with any of the anthropoid apes. The large size of the head tells decidedly against such a hypothesis. The toes seem, indeed, more easily and widely separable from each other than in modern Europeans; but this is a common characteristic in the Far East, where, *e.g.*, carpenters will often hold a board steady with their toes, whilst their hands are occupied with some tool. I do not see that, in the specimen now under exhibition at the Westminster Aquarium, the great toe is more fully opposable to the other toes than in human subjects. The growth of the hair will also seem of little importance if we reflect that among certain tribes —*e.g.*, in Australia—the backs of young children are densely covered with hair half an inch in length.—I am, &c.,

A GRADUATE.

THE "ADVANCEMENT OF MEDICINE BY RESEARCH."

To the Editor of the Journal of Science.

SIR,—When the Society for the Advancement of Medicine by Research was constituted Sir William Jenner is reported to have said that "there was no desire on the part of those who had worked in the formation of that Association to evade the law; there was no desire on their part to ask for a repeal of the law, or even for any modification of it."

Commenting on this statement, you very justly declared that if those words really limit the objects in view, "we scarcely see that the Association has any well-marked *raison d'être*." But now I read in the medical papers of "material assistance on behalf of harassed experimentalists whose efforts in the cause of discovery are at present hampered by the miserable restrictions imposed by the Vivisection Act." This sounds healthy, but it is sadly out of keeping with Sir William Jenner's opening address. —I am, &c.,

GRADUATE.

THE ELECTRO-MAGNETIC THEORY OF THE UNIVERSE.

To the Editor of the Journal of Science.

SIR,—You have doubtless met with some notice of the memoir contributed to the New York "Journal of the Telegraph," by H. Raymond Rogers, M.D., who considers "magnetic inter-action" as being the "key to the whole subject of universal force and to the methods of its operation."* Without going more closely into the details of this theory it seems to me very defective, as throwing no light whatever upon either the origin of the planetary bodies, or the power which set them in motion. It presupposes, *e.g.*, the sun and the planets made and and endowed with magnetic force, and then set in motion, gravitation being thus generated. Dr. Rogers speaks of "the motions of the celestial spheres, due to a *primum mobile* wholly undemonstrable." Here, therefore, we have the greatest cosmic facts either relegated to the realm of miracle or at least left utterly unexplained. I am surely warranted in asking whether Science is benefitted by such a hypothesis?—I am, &c.,

A LUCRETIAN.

AN APOLOGUE.

To the Editor of the Journal of Science.

SIR,—Perceiving that you do not espouse the views on higher education just now fashionable in your country, I venture to send you a short apologue which may convey a lesson:—

There were two nations, each in want of gold. The one, which we call A, set to work seriously to mine for gold, and to search for it in ores and furnace-products.

The other nation, B, did nothing of the sort. But instead it founded an assay-office in every village and a mint in every town, so that all existing gold might be "hall-marked," and might bear the royal imprint. Which nation increased its stock of gold?

* Our present issue contains an article which takes views not very dissimilar from those of D. H. R. Rogers.

The interpretation is not hard. The "gold" is trained intelligence. The two nations are mine and yours. Your mints and assaying offices are your examining bodies. Our gold-mines are our schools of research.—I am, &c.,

F. MEYER.

[The case is worse against us than our correspondent makes it. The assayer, though he cannot increase the quantity of bullion, yet distinguishes correctly fine gold from base metal. Our mental assayers reject the finest gold along with the pinchbeck, and "pass" the 8-carat articles.—ED. J. S.]

POPULAR INSTRUCTION.

To the Editor of the Journal of Science.

SIR,—You have often remarked the ignorance of the cultivated classes, and even of public instructors, concerning the natural sciences. I met with a striking instance of this the other day. Taking up by chance a periodical addressed to young ladies, I found in one of the articles a slug referred to as an "insect,"—not merely once, but repeatedly! If such darkness is in the teachers, what is likely to be the state of the taught?—I am, &c.,

"THOROUGH."

NOTES.

Our attention has been called to the subjoined paragraph in the "Secular Review." If a simple record of facts it is of capital value, as proving that some dogs have a conception of number at least up to twelve, which is more than can be safely said of some human races:—"A lady friend of mine has a bull bitch that is very wise, and is a Neo-Malthusian. She one day gave birth to fifteen pups. Counting her offspring, and then her feeding apparatus, she saw at once that there was a surplus population. Fifteen in family; food supply for only twelve; no workhouse for the three little extras. The venerable Malthus could not have solved the difficulty more promptly. Counting up her teats once more, and also her offspring, she selected three and put her sharp teeth through their brains, and by their prompt and painless extinction made a manageable and happy family."

The "Journal of the Royal Microscopical Society" for December, 1882, contains a translation of a paper giving a full account of the methods of microscopical research in use at the Zoological Station at Naples. The collection of formulæ for killing, hardening, and preserving is very extensive: the properties of the various solutions are discussed at considerable length, and full manipulative details given. Mounting in balsam seems to be the mode of preservation most in favour at Naples, and the whole series of processes employed aim at getting rid entirely of the water contained in the tissues, and replacing it with a preservative medium. When the absolute permanency of balsam preparations is taken into consideration, and also the fact that specimens from the establishment are transmitted to all parts of the world, there is a good reason for the adoption of balsam-mounting almost to the entire exclusion of other methods. The preparations of marine animals issued by the Zoological Station are well known to histologists for the perfect manner in which the processes of staining and permanently mounting are carried out, and also for their great biological value. The paper is the natural outcome of an institution which has no secret processes to withhold, and whose only object is research and the widest possible diffusion of the results. The translation will prove of the greatest value to English histologists, and furnishes another of the numerous instances in which the Royal Microscopical Society has placed translations of important foreign papers in the hands of its fellows as speedily as possible after their issue.

A museum is to be erected in connexion with the Owens College, Manchester. We hope the promoters will avoid the common error of expending the bulk of their funds on bricks and mortar, and having to depend upon chance donations for the contents of the establishment.

M. Boussingault ("Comptes Rendus") gives determinations of nitric acid and of ammonia in snow water from elevated points in the Alps. That from Neige du Velan (3760 metres) showed —nitric acid, 0; ammonia, 0·10 m.gr. per litre. Glacier of Kaltenwasser (3560 m.), and that of Palu (3000 m.), no nitric acid and no ammonia.

In a paper read before the Society of Arts, Mr. W. A. Gibbs, the inventor of the agricultural drying machine, speaking of the treatment he has received, said:—"If I had been a timid man, I should have been daunted; if I had been a poor man, I should have been ruined; if I had been a sick man, I should have been killed by this injustice."

The past autumn has been one of the driest on record in the north of Norway,—a contrast to the floods which have prevailed in England, France, Germany, and Italy.

A herd of the caaing whale (*Phocena globiceps*), estimated at near 20,000 head, has appeared among the Loffoden Islands. About 1500 have been caught in a single fjord.

Dr. Herdinger ("Deutsche Medic. Zeitung"), in a recent official report on deaf-mutism, shows that the offspring of consanguineous marriages afford 10 per cent of the cases, in addition to 5 per cent of the total cases, of blindness, and 15 per cent of those of mental disease.

Prof. T. Rupert Jones, F.R.S. ("Geological Magazine") sums up the causes of the sudden destruction of large numbers of marine animals. The principal are—efflux of fresh water from the land, volcanic agency and earthquake waves, hurricanes, excess or deficiency of heat in shallow water, diseases, and lightning.

Dr. Tuke, in a Lecture lately delivered before the Edinburgh Health Society, remarked that one of the great causes of overstrain in early youth was the vicious system of offering prizes for competition. It deflected the mind of the child from the main aim and object of its study, and often defeated the object which it was hoped to obtain.

M. E. Heckel (who must not be confounded with Prof. Ernst Haeckel of Jena) still denies the action of insects as factors of brilliant colouration in flowers. He brings forward no new facts, but says—"I persist in admitting [he should say in maintaining] that fecundating insects are not in any manner the cause of the

luxuriance of the floral system in some Alpine species." He seeks the cause in the solar radiations, which are more intense on the mountains than in plains.

At a recent meeting of the Midland Institute of Mining Engineers the new process of getting coal with lime-cartridges, in place of gunpowder, dynamite, &c., was discussed. It has given unbounded satisfaction in Belgium, Austria, and in many mines at home, the percentage of round-coal being more than doubled, whilst danger to the men is entirely excluded. The only failure was at Aldwarke Main, where the coal, being elastic, gave way without being ruptured.

Prof. O. Wendell Holmes has resigned the Chair of Anatomy at Harvard University, which he has held for thirty-five years. He intends, it is said, to devote his time to literature!

The annual yield of gold from the mines of French Guayana is about 2500 kilos.

According to Langer ("Kaiserlich Akad. der Wissensch.") the fat of children contains 67 per cent of oleic acid, 28 of palmitic acid, and 3 of stearic acid. In the adult the oleic acid has risen to 89 per cent, whilst the palmitic and stearic acids have fallen respectively to 8 and 2.

The poison of the rattlesnake, according to Dr. Ott ("Gazette Hebdomadaire"), kills animals by arresting the movements of the heart, and producing a considerable reduction of the arterial tension.

According to Dr. Perardon the lower animals are less susceptible to the action of resorcine than is man,

A M. Gourdin writes to the "Gazette de Cambrai" that the lightning fell three times upon a tree by the roadside from which he was only eight paces distant. He lay down on the ground, and covered himself with a woollen umbrella. The tree was cut up, and he was "coiled round with flames and completely electrified." He considers that he owes his safety to his presence of mind and his woollen umbrella.

In New Mexico there is another species of *Heloderma* allied to the poisonous lizard, *H. horridum*, recently brought to the Zoological Gardens. The species in question, *H. suspiosum*, is found by Prof. Snow not to be poisonous.

According to M. Hervé-Mangon the annual production of milk in France would form a stream of a metre in width and one-third metre in depth, flowing for the entire year at the speed of 1 metre per second.

Prof. H. A. Howe ("Kansas Review of Science") concludes that the comet of last autumn is not that of 1843, and that there is no danger of its falling into the sun at any time.

M. Risler ("Comptes Rendus") finds that the wheat plant never grows on any day when the temperature of the air in the shade does not, for some hours at least, exceed 43° F.

Mr. D. Eccles assumes that matter and its states are an illusion having no existence outside of mind, which is the only real existence.

M. Ch. Brongniart ("Comptes Rendus") describes a fossil orthopterous insect discovered in the coal-beds of Commentry. The insect in question, *Titanophasma Fayoli*, makes a near approach to the living Phasmidæ, and is 10 inches in length.

M. L. Vaillant ("Comptes Rendus") describes *Eurypharinx pelecanoides*, a deep-sea fish, caught off the coast of Morocco at the depth of 2300 metres. This fish is of an intense black colour; its locomotive organs are quite rudimentary. The enormous mouth and pharyngeal cavity form, as it were, the aperture of a funnel, of which the body constitutes the stem. Digestion is supposed to be effected in the mouth.

M. M. Mendelsohn ("Comptes Rendus") finds that, for a given weight, the mechanical work of a single contraction of a more excitable muscle is greater than that of a muscle whose excitability is normal. The latter, however, is less rapidly exhausted.

M. C. de Merejkowsky has presented to the Academy of Sciences a study on the *Suctociliata*, a group of Infusoria intermediate between the *Ciliata* and the *Acineta*.

MM. G. Pouchet and J. de Guerne ("Comptes Rendus") have studied the Malacological Fauna of the Varanger Fjord. The character of this fauna is distinctly Arctic. Of 94 species of mollusks, 63 are common to Greenland; 55 to Spitzbergen; 42 to Nova Zembla and the Kara Sea; and 41 to Behring's Straits.

A being is now under exhibition at the Westminster "Aquarium" which its owner, Signor Farini, asserts is "the 'missing link' which the late Mr. Darwin was unable to discover in the chain of evidence connecting the direct relationship between man and the monkey. This link assumes the form of a female child, about six or seven years old, said to have been discovered in Burmah. She looks, and is, a very intelligent child, who, although only a few weeks in England, speaks and understands English with ease. The head, which is set well upon the shoulders, and is in size out of all proportion to her body, is thickly covered with hair, which grows down to the eyebrows, which overarch eyes of extraordinary depth and penetration. The arms, legs, and body are covered with thick hair at least an inch in length. In support of Mr. Farini's theory that the little creature is but half human, it may be stated that she has a double row of

teeth in the upper jaw; that she can in the hollow of her cheeks stow away food to be eaten hereafter, as the monkey does in his 'pouches'; and that the fingers and toes bend backwards and forwards to the same extent, and with equal ease." Concerning the above description, quoted from a contemporary, we must remark that Darwin did not seek to discover a link between man and the anthropoid apes. The possession of a double row of teeth in the upper jaw is no more a simian than it is a human characteristic, and proves nothing. It is incredible that a creature lower in intelligence than man should learn in a few weeks to speak and understand English with ease.

According to "Light," Lord Ingestre took a book at random from the shelves, and holding it closed behind his back asked Didier to read the first lines on page 27, and Didier succeeded in so doing.

In an address delivered by Mr. Ivor MacDonnell, and reported in "Light," we regret to find the following misuse of language:—"The general public laughed heartily at *biological* displays."

We at one time suspected Spiritualists of having "Anti-Vivisectionist" leanings. We are glad therefore to note that "Light," in accordance with the expressed wishes of its principal supporters, has declined to open its columns to a discussion of the Vivisection question. This looks healthy.

The following clause in the proposed new tariff of the United States may be pronounced a somewhat one-sided tax on knowledge:—"Philosophical apparatus and instruments forty per centum ad valorem; provided, that any philosophical apparatus and instruments imported for the use of any society incorporated for religious purposes shall be subject to a duty of only fifteen per centum ad valorem." We must confess ourselves unable to see either the justice or the utility of the addition impost of 25 per cent thus thrown upon non-sectarian colleges and upon societies incorporated for purely scientific purposes.

According to Mr. S. Lockwood ("American Naturalist") the male of *Lepus Bairdii* has large teats and udders, and suckles its young ones.

Prof. Ferrier, F.R.S., is about to bring out a new and enlarged edition of his work on "The Functions of the Brain."

M. Proust, in a communication to the Academy of Medicine (October 31st, 1882), makes the startling assertion that there are still in France 219,270 houses without a window.

We are glad to learn that the American journal "Science" is about to be revived. Mr. S. H. Scudder, the eminent entomologist, is the appointed editor.

We understand that the Chemistry Class-room at the new

City Schools is so constructed that the voice of the master will be scarcely audible.

A meeting was held on the 20th ult. for the purpose of organising a national fish-culture association.

Prof. L. Miall delivered a Lecture on the 21st ult., at the London Institution, on "Dominant Forms of Animal Life."

Dr. H. R. Rogers refers gravitation to electricity, and maintains that "the *prime factors* in the problem of Universal Force are: (1) The motions of the celestial spheres due to a *primum mobile* wholly undemonstrable; and (2) *the magnetism inherent in these spheres.*"

We are very sorry to learn that the proposed meeting of the British Association in Canada meets with objectors. We might reasonably hope that men of Science would rise above that narrow, parochial spirit, which is one of the gravest defects of the English national mind.

We were mistaken in announcing the death of Prof. Palmieri, of the Vesuvius Observatory. The death of a nephew and namesake of the illustrious seismologist had given rise to a false report.

A subscription for the erection of a monument to Darwin has been opened at Paris, under the auspices of the Academy of Sciences. The Committee, however, make the reservation that their act does not imply any appreciation of "the general doctrines, scientific or philosophical, of the illustrious defunct."

It is with deep regret that we announce the unexpected death of our friend and contributor, Mr. Robert Ward, of Newcastle-upon-Tyne. The deceased gentleman had been engaged in preparing for publication the reminiscences of his distinguished relative, Bewick. The work was to have been illustrated with a selection of engravings from the original blocks. He enjoyed in this task the cooperation of Miss Bewick, who, we hope, will be able to complete the undertaking.

Dr. L. Kretzschmar (*Botanische Zeitung*) shows experimentally that the reagent used by Loew and Bokorny for distinguishing living from dead protoplasm (*i.e.*, a very dilute alkaline solution of silver) acts alike with both, and that the alkali alone, even in the absence of the silver, kills the protoplasm.

Dr. Schadenberg, on an exploring journey in the southern Philippines, has met with a new species of *Rafflesia*, the flower of which measures 30 inches in diameter. Its odour is unpleasant.